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STRACT

This report presents the content of the AERA lloquium, held in November 1968, to enable the educational research mmunity to assess and deploy its resources, coordinate research forts, and obtain more effective communication within the field and yond it. Several outstanding scholars were invited to present pers on their studies of the social systems and communication tworks of scientific communities, and to help elucidate the nature d characteristics of professional behavior of educational searchers. William Garvey and his associates described the more pical communication channels in educational research. William sley and David Lingwood examined the concept of invisible colle a way of examining the community of educational researchers. nald Corwin and Maynard Seider reported on their interviews of holars who have examined the institutionalization of science. rren Hagstrom and Norman Storer each examined the educational search community, using conceptual frameworks that have evolved om their studies of the social structure of the hard sciences. ese authors were joined by selected leaders within the educational search community who discussed the papers and made recommendations the research community in general and to AERA specifically. A list colloquium participants and footnote references are provided. uthor/DB)


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A Proposal to Improve the Social and Communication Mechanisms in Educational Research

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The Educational Research Community: Its Communication and Social Structure

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April 1970

U.S. DEPARTMENT OF
HEALTH EDUCATION, AND WELFARE

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FORWARD

This book is the result of a set of activities systematically undertaken by a voluntary professional association in an effort to better understand the social structure and communication processes of the community which it serves. It should be considered as a preliminary statement of the insights gained by the American Educational Research Association in its attempt to learn how researchers can intervene more effectively in the development and direction of their own field. This is, we believe, the first time that a professional association has reached out to convert knowledge gained in another specialty--here, the sociology of science--to its own use in fostering improvement in an emerging field.

The impetus for the study of the educational research community derives from several sources. Educational research, a young field which has been evolving in a traditional manner, is now confronted with urgent demands for specific solutions to the problems of education that are exploding in classrooms and on campuses. Politicians and concerned citizens are demanding greater payoff from educational research in terms of sufficient information for attacking a range of urgent problems.

At the same time, pressure is coming from within the community. AERA, like many professional associations in the behavioral and social sciences, has its black caucus and its dissident younger members who desire more relevant and imaginative research and development work. But the knowledge base on which successful educational practices can be built is not yet adequate to sustain major reforms.

In order to make any headway toward establishing such a base, the educational research community must be able, better than in the past, to assess and deploy resources, coordinate research efforts, rapidly disseminate findings, and maintain effective communication within the field and beyond it. Otherwise, the shape and direction of educational research will be determined by nonresearchers as indicated by the lack in educational research of the mutually accommodating relationship with the federal government that has characterized the natural and physical sciences. In recent years, priorities for research programs have been formulated almost entirely within the Department of Health, Education and Welfare, with little consultation from the field. More powerful signals must be heard from the field itself if a proper balance is to be maintained.

To learn how the educational research community can be strengthened, we have sought models in other fields of scholarship, particularly in the so-called hard sciences, where the influence of individuals or groups of researchers on the direction and priorities of their fields has been well documented. There are fields whose social structures and communications mechanisms are sufficiently sophisticated for sociologists to be able to study the forces and behaviors operating in them. It was the central strategy of our study to invite several outstanding sociologists of science, as well as others, to examine the social systems of educational research with the same end in view, to elucidate the nature and characteristics of the professional behavior of educational researchers. Whatever changes that will be proposed must be based on the observations and analysis of the present state of that community.

We asked William D. Garvey and his associates at the Center for Research in Scientific Communications at the Johns Hopkins University to undertake studies of the more typical communications channels in educational research: the annual meeting of AERA; the Journey into print of materials presented at the meeting; and the production of current journal articles. These detailed base line data about the way scientific information is exchanged in the field has been compared with similar information about several other, better established fields in the "hard" and "soft" sciences.

William Paisley and David Lingwood of the Institute for Communication Research at Stanford University were asked to apply the notion of the invisible college to the educational research community and to suggest how AERA might best relate to and exploit an invisible college developing in this field.

Sam D. Sieber and Caroline Persell of the Bureau of Applied Social Research at Columbia University devised a strategy to explore how insights derived from the sociology of science could be applied to the development of educational research. This was carried out and reported on by Ronald G. Corwin of the Ohio State University and Maynard Seider of the University of Wisconsin through lengthy, unstructured interviews with scholars who had done work on the institutionalization of science. From their own theoretical viewpoints, those interviewed were asked to comment on their perceptions of educational research, the genesis and nature of its problems, and the solutions that seemed most likely to speed its development.

To bring together and synthesize these studies, and to provide a forum for discussion, a two-day colloquium was held in November, 1968. Two additional formal papers were commissioned from Warren O. Hagstrom of the University of Wisconsin and Norman W. Storer of the Social Science Research Council. A selected group of sociologists of science, communications researchers, and leaders within the educational research community, came together to examine the findings of the studies, to pinpoint the critical problems in the field, and to make recommendations to the long-range planning committee of AERA. While the participants were not called upon to design specific activities for the Association, their counsel was to be carefully weighted.

The content of the colloquium is reported here in a way that we feel avoids both the dryness of a brief condensation, in which much matter and spirit can be lost, and the tedious detail of a full transcript. Some necessary reorganization of the original transcript was done in order to cluster the comments around the major themes and provide continuity. We have tried to represent faithfully the thoughts and opinions of the colloquium members cited.

The long-range planning committee of the AERA is currently preparing its final report of recommendations for action based on the studies and discussions in this book. My own assessment and conclusions will be found in the final chapter. The book as a whole is, of course, both a study of and an exercise in the communications process of the educational research community. Our hope is that it will serve other purposes besides that of providing AERA with guidelines for improvement.

For one thing, we have tried to show educational researchers that certain sociological methods and findings are applicable to their field. Conversely, we have brought the language and problems of educational research to the attention of some sociologists, with the hope of stimulating cross fertilization between fields. Workers in educational research should gain a clearer view of their own community, and practicing educators can broaden their understanding of the present and potential roles of educational research.

In the practical, day by day accomplishment of these roles the real work now remains.

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Many persons and institutions helped guide and support the activities that generated this book. The Research Branch of the Office of Education provided the funds for the study conducted by Corwin and Seider, and for the colloquium itself. Contributing their time and patient interest to the interviewers were Bernard Barber, Joseph Ben-David, Terry Clark, Stephen Cole, Diana Crane, Barney Glaser, Norman Kaplan, Thomas Kuhn, Paul Lazarsfeld, Simon Marcson, Herbert Menzel, Anselm Strauss, and Harriet Zuckerman. We are deeply grateful for their wisdom.

To all the participants in the colloquium, whose names and affiliations are listed separately, we owe a debt of thanks for their contribution at the time, and their continuing interest. In addition, this writer has profited from discussions with Norman Storer and Henry Riecken. Special thanks are due to Ronald Corwin and Maynard Seider who responded on very short notice to a task at a very critical time.

For competent assistance in the organization of the colloquium and its aftermaths, I wish to thank Mrs. Ruth Guttadauro, Miss Penelope Tasche, and Mrs. Marianna Diggs.

But more than anyone else, this document was shaped by Mrs. Sara Mitter who collaborated in the preparation of the first and last chapters, and did the technical editing throughout. My deepest appreciation is reserved for her.

Richard A. Dershimer

THE COLLOQUIUM: A RECAPITULATION

The chief purpose of the colloquium was to help inform leaders in the field of educational research, particularly those who are officers of AERA, about effective ways to reshape the social organization and restructure the communications networks in the field. Opening remarks by the colloquium chairman, Richard Dershimer, by David Krathwohl, the 1968 President, and by John Goodlad, the immediate past president of the Association, made it clear that, while the ultimate concern was to find ways of achieving this needed redirection through the Association, the discussion should not be limited just to proposed new roles for professional organizations.

The community of educational researchers, it was noted, has not been subjected to systematic study. Except for the two analyses of communications networks made by Garvey and Lingwood*, little precise information exists about the behavior of persons in educational research. For this reason, studies of scientific fields were examined for data that could supplement the hunches of AERA officers about how to improve the workings of their own field. The relevance of this step depended, of course, on establishing whether and how educational research is like other fields that have been subjected to study.

Garvey maintained that, on the basis of the criteria he and his colleagues used to describe the communication structure of the nine fields they are studying, educational research was behaving as much like a scientific field as any of the social sciences, including sociology and psychology. Educational research exhibited most of the same communication media, organized in similar patterns as other disciplines. There are, however, some significant differences among disciplines, as mentioned in the Garvey, Nelson and Lin paper.

Storer agreed that a discipline cannot exist apart from its knowledge base, but suggested that it is quite possible to develop a social organization that <u>looks</u> like a discipline, simply on a foundation of shared faith that a knowledge base <u>will</u> emerge in time. He felt that this is the case with educational researchers.

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*Lingwood's sociometric study, "Interpersonal Communication, Scientific Productivity and Invisible Colleges," was reported at the meeting but not included in this document. Reprints may be requested from the author.

Although he agreed with Storer, Hagstrom saw some serious weaknesses in what he called the cultural base of the field. Educational researchers, like most other behavioral scientists, must rely more than the "hard" scientists on consensus to establish knowledge. The weakness in the culture of educational researchers is that practitioners play a more significant role in the formation of consensus than is the case in most other behavioral fields. He documented his observations by describing how the concept of service and the role of teacher preparation play a dominant part in colleges of education.

Bidwell concurred with Storer and Hagstrom that drawing comparisons among communities of researchers could provide important insights. To the definition of "community" as the density of interaction within given boundaries, he added a qualitative dimension. One of the most important aspects is a sense of identity that develops during the socialization process in graduate education. Bidwell felt that the educational research community would continue to remain quite constricted until graduate students in disciplines other than psychology could be successfully encouraged to think of themselves as educational researchers.

According to Corwin's observations, there seemed to be a relationship between the number of scholars and researchers in a specialty within a discipline and the degree of acceptability of their studies to the remainder of the discipline. He reported that several of the persons whom he had interviewed were convinced that a critical mass of respected people had to assemble before a subdiscipline or a specialty had any meaning. Besag doubted whether there was a critical mass among those studying education in any of the social or behavioral sciences other than psychology.

Marcson ventured that educational research might more appropriately be compared with communities of engineers, developers, or technologists rather than with researchers. Gage thought educational research might not be a unitary field but a mixture of things, just as the field of architecture is a mixture of esthetics, engineering, economics, and politics. Such fields can be seen as hybrids, or collections of specialties. And yet he felt that educational research does have a unity to it even though it lacks the coherence of a discipline. Marcson, agreeing with Gage's analogy, pointed out that many of the presently recognized scientific fields do not have the unity that is attributed to disciplines; space science, for example, is not a coherent field.

The chairman directed the attention of the group to a description in Storer's paper of educational research as a "conjunctive domain," and asked him to give a brief summary of the concept.

Storer explained that he was trying to find a way to classify a field of study that is focused around broad areas of immediate social concern. Several examples come to mind: Space science, medical research, agricultural research and, of course, educational research. Agricultural research, a typical example, includes segments of several basic disciplines like entomology, plant physiology, and pathology,as well as the more applied or developmental fields of soil physics, plant breeding, and food processing. The clustering of the efforts of basic researchers, applied researchers, and developers around problem areas may be thought of as a conjunctive domain. However, the implications of this definition were not pursued by the participants.

Corwin circumvented the issue of whether educational research was analogous to a discipline by arguing that research fields have been organized around critical questions, conducted outside of disciplines, and housed in professional schools. Several of the experts he interviewed had compared education to engineering or nursing. He opted for the identification of educational research as a "field of research" or a "field of study." Bidwell added that it is an enormously pluralistic field in its structure, attributes of members, the nature of funding, etc. Gage and Travers concurred, pointing out that educational research and development now have all of the rewards, though perhaps to a lesser degree, of many of the highly sophisticated sciences, including recently established federally-funded institutions for research and development.

Travers cautioned that the group should not attempt to become too explicit in its definition. He sensed that at this stage in its evolution, when educational research was growing rapidly and adding a great many new people and institutions, it was better to leave the definition open ended. Extremely significant findings quite likely could materialize from individuals who easily could be excluded from any present definition of the field. Both David and Bidwell agreed. They urged the participants to think of what the field was becoming or what it might evolve into, particularly if all of the social sciences began to contribute significantly.

Having concluded that the communities that could be identified as operating within the educational research field of study were sufficiently sophisticated and advanced so that analogies could be made with other fields of study, particularly in the behavioral sciences, the colloquium members turned their attention to an attempt to identify the major problems facing these communities. They fell into four categories: 1) problems arising from the institutional settings; 2) impediments to the socialization processes; 3) problems of defining the invisible college; 4) difficulties stemming from the concept of educational engineering and development.

1. Problems arising from the institutional settings.

Chief among the issues facing the community are those that relate to the institutional settings provided for educational researchers. Corwin introduced the topic by emphasizing that the persons whom he had interviewed returned frequently to conditions within schools of education. He was convinced that most of the problems facing the research community would have to be resolved at that level.

Smith agreed and pointed out that most people who hold a doctorate in education end up spending nearly all their time in the preparation of teachers. Few doctoral programs in the country actually prepare educational researchers or scholars. Hagstrom added that even those who are prepared for research emerge from schools of education with a closer relationship to practitioners than social scientists have. He quoted figures from his paper indicating that the majority of professors in schools of education have had professional experience in the public schools, whereas only twenty five percent of social scientists have had similar experience; that education professors spend significantly more time teaching students majoring in their own fields than do social scientists, and are more likely to do consulting or service work for school systems. They end up in the dilemma of being over-dependent on practitioners and yet feeling alienated from them.

Gage restated the problem as being that of reducing conflict between service to schools on the one hand, and building knowledge about education, on the other. That is, this problem has taken the form of a conflict between "research" and "service". To Besag, this predicament was best exemplified by the way deans of colleges of education evaluate the academic achievement of their faculties. Far too many administrators, he felt, are uncertain about standards of scholarship and do not give adequate recognition to educational researchers, particularly those from the disciplines. Goodlad agreed, suggesting that a stronger community of scholars was needed in order to counteract the ambivalence of schools of education about their responsibility for conducting what he called "conclusion-oriented" rather than "decision-oriented research"; in other words, the conflict between doing research attempting to add directly to knowledge and that aimed more at influencing the behavior of practitioners. Researchers, he felt, need support from their colleagues because they necessarily must become critical of the enterprise they are studying.

While some of the discussants pessimistically concluded that researchers could not be trained under conditions typically found in schools of education, Corwin pointed out that the new Ph.D. programs of research recently established in schools of nursing appear to be working quite well. There are reports that directors and instructors in these programs find distinct advantages for training their researchers within the settings that they will be studying. Storer concluded by stating that while a great many problems are inevitably found in all professional schools a larger number of these will be resolved if bright, capable people are doing that training. But different institutional arrangements, it was felt, would make better use of their talent.

2. Impediments to the socialization processes.

A point to which Bidwell returned several times during the discussion was that the socialization processes* are important in forming and shaping the characteristics of any research community. They may be even more important in education, he felt, because research on topics in education is frequently downgraded in universities, especially by the educational researcher's co-disciplinarians in the arts and sciences. Hagstrom concurred for three additional reasons: a) the lack of consensus about what is viable knowledge, b) ambiguities in the prestige hierarchy, and c) deficiencies in the reward systems. In his view, educational researchers are less able to identify and rank the great men and the institutions having the most prestigious departments in the field. Bidwell pointed out that this may be true because education is a highly diverse and complex field and the criteria for identifying competence or greatness are more complex than they are in some of the more unified fields of study.

Hagstrom was asked to comment on why he felt that the social systems of education were as blurred as he had described them. He responded by pointing out that professors in schools of education are quite likely to have changed specific fields after receiving their doctorates. Also, schools of education are very heterogeneous in their organization and often stratify across disciplines. Thus, professors of education are less likely to have their primary reference groups from disciplinary colleagues.

Garvey reported other explanations derived from his group's studies of the communications networks. For example, in their study of the annual meetings of nine associations, he found that AERA members were less familiar with the current and past work of the authors whose papers they went to hear than were members of any other association. The fact that 256 authors cited a total of 67 different journals to which they planned to submit their papers, the largest number they found in any field studies, led him to conclude that the communication networks are diffused. They do not encourage researchers to cluster around core journals, one way in which social systems are structured.

Bidwell declared that Garvey's studies reinforced his own suspicion that, for example, sociologists concerned with education are talking mainly to other sociologists. He saw the field of

*Bidwell defines socialization as the process whereby individuals internalize roles in specific social settings, developing a grasp of the system's behavioral norms, both written and unwritten.

educational research as having a weak centripetal force encouraging those scholars in the arts and sciences disciplines who are concerned with educational topics to turn to discipline-based rather than education-based colleagues for information, critical review, and approbation.

Reflecting upon what produces a centripetal force, Storer concluded that there must be a large enough number of respected men in any sub-discipline before a separate social system can be established. He referred again to the concept of a critical mass. But Bidwell cautioned that, as the social system of any sub-discipline or specialty becomes more recognized within the larger unit of the discipline, that is, as the specialty becomes more highly regarded, more members of that social system are likely to move back within the main stream of the discipline itself. To him, this process then becomes divisive for emerging fields like educational research. Yet in order to improve the quality of their product, researchers in education must remain strongly linked to whatever arts and sciences discipline is most germane to their own work.

Besag described the opposite side of the coin. Researchers seeking colleagues from whom they can obtain a competent response will exclude practitioners. However, since educational research is an applied field, it must stay close to the concerns and the views of practitioners. He feared that the field may grow so conscious of its purity that it could become sterile.

To Storer, the key element in the socialization process involves determining where young researchers can get the most competent responses to their work. Hagstrom agreed, also stressing the word "competent." His view of the most important aspect of any social system is the feedback it provides to the individuals concerned.

3. Problems of defining invisible colleges in education.

It was assumed by the colloquium planners that the characteristics and the influence of the invisible colleges in education would be discussed in depth. For several reasons, however, this turned out not to be the case.

While Paisley and Lingwood saw evidence of invisible colleges from studies they had made, their work was not sufficiently advanced to give a thorough description of who they represented and how they function.

Lacking precise data, the group attempted to identify invisible colleges from their impressions and experiences. Gage felt that one existed around the <u>American Educational Research Journal</u>, among its authors and consulting editors. When Paisley expressed doubt that the journal was well enough known yet, Gage tried to redefine the invisible college as standing for the network through which preprints of articles are distributed.

Garvey disagreed with the notion and called for a better definition of invisible colleges with particular attention to the differences between them and reference groups. He stated emphatically that he saw no evidence yet that invisible colleges exist in education.

4. The problem of education engineering and development.
More colloquium time was given to discussions of the concept of educational development and the problems of the developers than to any other topic. The importance of the question was established by David when he stated, "Unless you tease out what development is in education, you promise that the field will remain in that same state it has been." Lumsdaine too saw the problems of educational developers as being critical. He claimed that there has not been nearly enough analysis of product performance in various settings with students of various characteristics. To this McLean enthusiastically agreed.

Smith emphasized the importance of development from a historical standpoint. He suggested that the research that has changed education in the last 150 years has really been development. Thorndike's studies of arithmetic, as only one example, had a strong influence on the nature of arithmetic books. Spelling books were completely reconstructed from studies of the frequency of occurrence of words, word counts, and analyses of this nature.

In Gage's view, almost everything in the schools today--thousands of textbooks, visual aids, and other materials--has been the product of development. He wondered why so much attention was being given now to development, since it was nothing new. Perhaps the difficulty was simply that much of the early work done under the name of research was in fact development. Did persons like Thorndike, or Rugg, or Gates ever report in the literature questions such as, "How can I rearrange textbooks to be most effective?" Gage had not seen such reports, but Travers maintained he had read some early writings of this type by Thorndike and Gates. Some thought that it was the nature of the communications systems of the time that had allowed many such reports to fall into oblivion. But Gage still challenged the group to clarify, first, whether such acknowledged products of development as Individualized Prescribed Instruction are publishable in the traditional sense; and secondly, whether the norms of the social systems of developers include publishing. As he claimed, "We know what researchers do, they write and publish papers and report them at meetings. What do developers do? They don't write and publish papers."

Several persons rose to refute this contention. The attempt to arrive at a better notion of what developers do involved an effort to define development in education more clearly.

Hagstrom asserted that his observations had not yielded a clear idea of what developmental jobs are like. McLean proposed that development was "the production of working prototypes." These could be products of many kinds, like new curricula, for example, or computer terminals. To Lumsdaine, development is the research concerned with the creation and improvement of products and byproducts in education. To Schutz, development is the reliable attainment of outcomes at the least cost and in the least amount of time. Development is concerned with processes as well as products, according to McLean.

David pointed out that other fields--urban studies, for example--have similar difficulty in defining development. In a review of the R & D programs of the Department of Housing and Urban Development, the consultants could identify the developmental activites in the engineering sections much more readily than they could in the behavioral and social science sections. Marcson cautioned that it is often not easy in engineering, and that there is considerable blurring of the definition between development and research even in physics.

Schutz saw research and development as being quite distinct from one another in education. Research is much more oriented to "verbal manipulation"--that is, to definitions, labels, and hypotheses. Development is concerned with products, operations, and demonstrations that involve people. There emerge different roles and reponsibilities for the individuals involved.

But who are the developers, and where are they? Some wondered if the senior staff in the regional educational laboratories are developers or whether they are closer to academic researchers in orientation. Schutz was convinced that the latter was the case, but pointed out that this was the result of how and where they had been trained. As Bidwell argued, educational developers may suffer from discontinuous careers; they must perform like academicians rather early in their careers, or be judged as incompetent. Engineers in the hard sciences, on the other hand, Marcson commented, can work in virtual obscurity for five years if they are building something magnificent like a high speed accelerator. Could educational developers, he asked, spend five years putting together an analogous product, without publishing, and still survive professionally? There was general agreement that this would be very difficult.

And yet, five years was felt to be a reasonable estimate of the length of time that will be required by the regional labs and other such new institutional settings to breed a new generation of engineers. McLean pointed out that it had taken about that period of time for the new curriculum developers, like those who designed the Physical Sciences Study Committee program, to come into their own.

Marcson suggested that the problems of this group were similar to the problems of engineers in general. He pointed out that engineers seek and use a larger range of rewards and feedback systems than do academic researchers, because of the nature of their products. To them patents are frequently more relevant than publications. As he remarked, "You can't spread a concrete engineering product around for review." However, he cautioned that it is a myth that engineers do not publish at all. Industrial developers have well established reference groups, invisible colleges, formal channels for publications, and active annual meetings at which they regularly report on their work.

McLean disagreed, however, with the notion that engineers and developers in education could be so closely identified with engineers in general. He was supported by Nan Lin who pointed out that the basic difference between the broad category of engineers and those specifically in education is that the former can measure products over a short-term period. In education this is very difficult. And David cautioned that development in a research field that is related to practitioners, like medicine or education, has special requirements and the number of conclusions that can be drawn from comparisons with engineering fields is limited.

Goodlad noted that certain other categories of workers, for instance, innovators, should not be confused with developers. Innovators generate products but do not engage in any systematic evaluation of their work. To Marcson, such people are the inventors. They differ from engineers and developers in that they do not report on their methodologies and are likely to work outside of an organizational context.

If such is the case several group members reasoned, then a convenient way to identify developers might be through institutional settings. The first to be mentioned were the regional educational laboratories and the research and development centers. A discussion of the products issuing from these sources and the behavior of the persons involved seemed to conform to the previous definitions. Gage cited microteaching as a specific example and mentioned that several persons in a R & D center and regional labs who were responsible for its development had gone for many months without publishing anything about it in recognized journals. But Travers, Goodlad, McLean, and Schutz all argued that they could cite as many or more individuals turning out products in the centers and labs who could not be called anything else but researchers.

Paisley identified a large group of developers in what are referred to as "Title III" centers. Few were in any way connected with the research community, but were more responsive to local boards of education and federal bureaucrats. As a director of an ERIC center, he was aware that these men did publish, but in technical reports rather than journal literature. But some members of the group suspected that Title III people were closer to being inventors than developers. No matter how much the development component of "R & D" continued to defy definition, Corwin concluded, its importance was inevitably growing. The federal government and particularly the Office of Education have put great stress on product and system development. For this reason it can be assumed that increasing attention will be given to identifying the agents and products of educational development.

Having brought to light some of the major factors that limit the effectiveness of the community of educational researchers, the colloquium participants set to work to suggest how these problems might be alleviated.

Storer felt that since educational research is not provided with an integrated body of knowledge awaiting development, the critical question is whether other devices can be manufactured to create a self-sustaining field, in which groups of researchers will work at given substantive issues. Five ways to bring about this development were discussed: 1) revising the communications systems; 2) providing better reward systems; 3) improving the social systems; 4) increasing the social utility of research results; and 5) influencing the policies of the federal government.

Of these, the communications network received the most attention. Paisley saw a centripetal force developing around and from a communications network. Make the network easy to use, for the writer as well as the reader, and more and more people will cluster around it. In Paisley's view, the present network in educational research was inadequate by almost any criterion. It operated most effectively for the inner core group of psychologists and educationists but had little to attract sociologists and others from the social sciences. Garvey concurred, pointing out another weakness: the considerable time lag his studies revealed between the date that research projects were completed and the time that the first reports of the projects appeared in professionally referred journals.

But David cautioned that it was not enough to concentrate on facilitating the existing networks. Rather, top priority should be given to redesigning all systems to meet new objectives, especially that of enlarging the perimeters of the core literature. Krathwohl interpreted these comments as pointing up the basic dilemma facing the community: to make the literature more inclusive to accomodate other disciplines and at the same time more exclusive, so as to improve the quality of research. To Bidwell, the problem of communication networks was part of a larger dilemma. A community must grow in size and complexity in order to develop the critical mass of effectiveness that had been called for earlier. But the larger the group grows, the harder it becomes for individuals to develop the sense of identity that is the cohesive force.

Without disagreeing, Storer maintained that the paramount issue was something else--the quality of the journals. Educational researchers must make their journals competitive with prestigious journals in related fields; nothing else would suffice if they hoped to print the work of recognized scholars.

Paisley advised that the annual meeting is very much a part of the communications systems, and called for thorough revisions of the purposes, organization, and format of this event. Paper reading sessions, he proposed, emphasize divergent attacks on problems; symposia stress convergence. Because an emerging field like educational research needs more of the latter, he urged AERA to engineer more carefully the content and contributors to each year's program. Travers was not convinced; he felt that researchers do have a sense of what is urgent and, if left to their own devices, will probably focus on the right topics. Furthermore, he saw that much of what Paisley wanted to change was traditional and ritualistic--but also harmless.

The effectiveness of the reward system also depends in part on how well the communications network is functioning. As Storer pointed out, the communications systems are in many ways the academician's monetary system: they must be working well for scholars to be well "paid" for their work. Pay in this case takes the form of appropriate feedback from the most relevant colleagues. Several voices claimed that the motives of all researchers are not so pure--that money and position are also important. But this aspect of the reward system was assumed to be well understood. Of more interest for this discussion were the subtler motivations. Bidwell, for example, emphasized that most researchers are trying to increase man's knowledge and frequently make conscious professional choices in order to add increments to knowledge.

But how do researchers decide what work merits such rewards? Consensus among the most competent colleagues is the accepted answer. This sets a very pragmatic criterion for the utility of any group or any social system in the sciences--its ability to make competent evaluations of the work of others. The group is more important in the social sciences, Hagstrom asserted, than in the hard sciences where a body of knowledge and set of standards are better established and understood. This difference accounts in part for the high rejection rates of 80% in the more prestigious journals in the social sciences, compared to a 20% rejection rate in physics.

Paisley remarked that the rules and standards of any field are informal in nature, and are not codified. Hence the socialization processes that introduce new members of the community to standards of methodology and professional conduct are important.

The attention of the group then turned to the third way that a new field could set up a centripetal force, through the improvement of the social system. Corwin's prerequisite, the development of a critical mass of competent men, was generally accepted, without anyone's attempting to establish the order of magnitude of the mass.

Bidwell tried to distinguish between a simple increase in the number of persons working on educational problems and the creation of a social system. He felt certain that many more sociologists nowadays are studying education, but they did not think of themselves nor interact primarily as educational researchers or educational sociologists. Lacking this identification, they further impeded the socialization of graduate students into the educational research community by downgrading the field. He asked how this tendency could be reversed.

In responding, Hagstrom, Storer, and others returned to already established points. Raise the standards; find more competent people to provide more vigorous interactions so that the work produced is judged as being of higher quality by an even larger segment of researchers in the behavioral and social sciences. Storer summarized by stating, "If you can't beat them (the established fields), join them. The only way is to improve the toughness of editorial standards for journals in your own field. This is a long slow process, and a lot of heads get cracked along the way, but it is the only way in the long run."

But Paisley would not let the matter end there. He pointed out that certain fields like aerospace research had sprung up in response to demands outside the academic community. In addition to the criteria already mentioned for the development of a centripetal force, he spoke of money as having paramount importance. If federal agencies could be persuaded to allocate funds only to selected people who had agreed upon standards and who could exercise tight control, he believed that a highly effective field of research could be built in ten years.

While no one in the group agreed unreservedly with his stand, several acknowledged the power of the government in establishing or altering research fields. Gage felt that new institutions established by the U. S. Office of Education such as the R & D centers and the regional education laboratories, have had considerable influence on research communities. Many new people are being attracted from other departments in universities and larger numbers of graduate students have turned to educational research as a career because of the creation of research assistantships and supportive environments.

Paisley turned again to such fields as the space sciences and urban education to support his next point, that special interest groups can emerge around research problems of social import long before research of any significance is produced. Gage reiterated the belief that the federal government sometimes attempts consciously to engineer communities in advance as one way of delivering data, findings, and conclusions on vital social and political issues. While high general interest in an area might induce many men to move into it, Storer conceded, something more would be necessary to sustain them over the several decades that it might take to establish the fundamental knowledge. In fact, Storer added parenthetically, the government might profitably spend its money in helping to build some of the mechanisms, like communication networks, that sustain communities.

In summing up the elements that would be necessary for the launching of a self-sustaining field, Hagstrom pointed out that the mature fields of knowledge. i.e.. the disciplines, had both a culture and a set of institutions. Inherent in the former were values, beliefs, and norms of established behavior. In the latter were economic units, professional occupations, and titles. To further the growth of a field, one could start with either side, he believed.

Assuming that such a self-sustaining field could be developed, what should it look like? Should it be an interdisciplinary field, a newly developed hybrid--something like Storer's conjunctive domain--or should it pattern itself after a discipline? Paisley felt that if tight standards were enforced, educational research could become a hyphenated discipline like social psychology or biochemistry in ten years. But standards would have to be enforced by the field itself. Aerospace research does not police its standards and is not considered a discipline; biochemical research does. Gage was skeptical about educational research being able to move in this direction. More than tight standards is involved in defining a hyphenated discipline, he cautioned--there must be laws, concepts, and other substantive aspects that bridge two fields.

Garvey shared Gage's skepticism about creating a interdisciplinary field, but did see educational research as the meeting ground for researchers from many fields. Many psychologists wish to leave psychology because they grow tired of experimenting with rats. On the other hand, educationists frequently identify with "hard science" psychology. An effective interdisciplinary effort might grow from such associations.

But corwin saw many obstacles to establishing an interdisciplinary field. For one thing, different disciplines use different methodologies. They have different ways of identifying problems. How can standards be established under these conditions? Interdisciplinary research might be easier to attain at the microlevel than at macrolevels, as Hagstrom suggested. Perhaps researchers from different disciplines could work together in specific institutional settings. Or, Corwin added, effective collaborations might take place at the elite level, among super-stars whose interests and knowledge transcend disciplinary boundaries.

He felt, however, that a hybrid had to emerge if educational research was to become effective. By a hybrid he meant a distinctive field of endeavor lodged between the disciplines and the world of the practitioners. McLean concurred by saying that educational researchers needed to begin developing their own standards independent of the disciplines. In Bidwell's view, a hybrid could result if quality improvements in schools of education encouraged the development of independent reward and social systems.

This led to the final major question: What could the American Educational Research Association do specifically to help shape the research community? The suggestions ranged from David's general wish that AERA might serve a broad range of academicians, developers, and practitioners to the quite specific recommendations of Garvey and Nelson for improving the annual meeting. For the most part, the comments focused on three issues: revising the publication program; broadening the audiences to be served; and attracting and holding educational developers.

Paisley's earlier statement, that men will be drawn to a field whose communication network is easy and rewarding to use, brought further comment. The difficulty in improving the AERA publishing network would be compounded, Garvey and Paisley pointed out, by the fact that recent studies have forecast ever-increasing costs for maintaining traditional journals and for initiating new ways of automating publication services. They both urged AERA to work more closely with federal agencies, but disagreed on the present value of the Educational Resources Information Center (ERIC), Garvey being critical of its performance so far.

The long time lapse between the completion of first research reports and their appearance in journals was Garvey's particular concern. He urged AERA to develop a variety of devices for announcing recently completed work. Travers saw a greater need for textbooks which give up-to-date summaries of research in highly specialized areas and spoke highly of two that he had seen dealing with attention phenomena and perceptual motor skill. Gage emphasized the need for a journal like Psychological Bulletin and unlike the present Review of Educational Research that would publish author-initiated reviews and evaluations of current research on topics formulated by the author.

But Lingwood cautioned that AERA should not be overly concerned with improving the communication networks for the highly productive men; they were well "plugged in" to the work of their colleagues and had less need to alter their communication links and information sources.

This raised the issue of whom the Association should be serving. Lingwood counseled AERA to focus on the practitioner but this suggestion won few adherents. Paisley suggested that AERA take greater responsibility for those persons next to the practitioners, like Title III personnel and the individuals responsible for research and evaluation in local school systems. David wondered why the Association could not serve several constituencies--with members from the disciplines, developers, and practitioners. Hagstrom thought AERA might give rewards to emerging groups without disciplinary status or to groups whose discipline would not yet acknowledge them--anthropologists of education, for example. But Bidwell again voiced his doubts whether AERA could provide the necessary rewards for men from the disciplines.

Lumsdaine voted resoundingly for the creation of a structure that would bring the researcher and the developer close together. Researchers, he asserted, have the knowledge and methods needed by developers to devise better educational products, and particularly to analyze product performance in various settings. McLean, Smith and Gage all concurred. But the proper relationship of the Association to the developer remained a problem. While few of those present opposed a marriage, there was wide disparity of opinion about the importance of the problem and how the marriage should be joined. Garvey warned that developers and technologists would drift off into another organization--if indeed they were not already there--if AERA did not make efforts to attract them. Gage urged that the Association provide channels and rewards for developers, such as special journals or prizes. As far as he could see, the present mechanisms were not helpful to them. As Garvey affirmed, AERA's reward structure was patterned after that used for scientists.

But could the two groups be brought together at all? All organizations contain developers and researchers in varying mixtures, Garvey declared. He urged the Association to use its mechanisms to emphasize those problems that have greater relevance for both groups, as a way of drawing them together.

According to David it is the size of an organization that sets the limit on its diversity. Marcson cited some specific associations wherein researchers, developers, and administrators exist side by side--the American Chemical Society and the Institute of Radio Engineers being only two. ACS selects its president alternately from industrial and "pure" research, and has a journal for every major audience. Marcson felt that developers in industry belong to scientific societies because developers are alert to what is happening in science and constantly seek out help from scientific researchers.

It was suggested, in sum, that AERA write a "scenario" for itself, establishing specific objectives so that it could inventory mechanisms and strategies available to associations. Paisley urged that the Association think of itself as a system and specify its objectives in immediate, intermediate, and ultimate terms. Only then will it be able to determine the cost/effectiveness ratios of various activities, and use maximum resources to move educational research forward.

PATTERNS OF EDUCATIONAL RESEARCH: REFLECTIONS ON SOME GENERAL ISSUES

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Over the past forty years, the field of educational research has grown in scope and complexity. This expansion has been accompanied by a host of problems reflected in several critical issues which have divided the educational researchers in recent years (Corwin and Nagi, forthcoming). There has been little research on the evolution of new fields of research, but it should be profitable to review these issues from a broader perspective.*

On the assumption that scholars who have actively studied other fields of scientific endeavor might shed some light on the issues facing educational research, fourteen scholars with an acknowledged expertise in the history and/or sociology of science were asked to serve as informants during unstructured interviews, lasting approximately one and one half to two hours.** They were told that the purpose of the interviews was to obtain their assistance in placing the immediate problems of educational research into a broader theoretical context. They were asked to reflect on more general sociological issues underlying the problems outlined for them. Prior to the interview, each expert was mailed material describing the field of educational research in general and the American Educational Research Association in particular*** together with a

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** See Appendix A for a list of those interviewed.

*** See Appendix

ten-page "Interview Guide" structured around several problems.*

(1) the "splendid isolation" of education (as Cremin refers to it);

(2) the low overall quality of educational research and the apparent need for more of the best minds from basic social sciences and related disciplines to do research on education;

(3) unprofitable relationships between basic and applied research and between research and service;

(4) the influence of federal bureaucrats in setting research priorities in this field.

The interview guide was used only as a point of departure and served primarily as a heuristic device to provoke critical commentary. In our difficult role of interpreter, we tried to state the issues as we had heard or read them stated by people closely identified with the field, while at the same time presenting these issues in a form that would be meaningful to social scientists, some of whom were unlikely to have an intimate acquaintance with recent events in the field of education. The discussion following is organized around a series of questions which were largely implicit in the interview guide.

STRUCTURE OF THE FIELD

The growth of the field both in scope and in differentiation** has opened up new possibilities for development and creativity, and

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* We do not claim to have fully demonstrated that these are in fact dilemmas in any rigorous sense of the term. We wanted to preserve what we saw as the prevailing conception of the issues, even at the risk of oversimplification and perhaps some distortion from a sociologist's point of view. We have no way of knowing at this time how widespread the awareness of these issues is throughout the educational research community.

** Over the past 40 years, the number of disciplines involved in educational research has increased from three to twelve; accordingly, there has been a considerable increase in the scope and complexity of inquiry (Tyler, 1965). A recent study reports that the relative number of proposals submitted to USOE from researchers outside colleges of education increased several times faster than proposals submitted by applicants located within schools of education (Sieber and Lazarsfeld, 1966). At present, the majority of research proposals submitted to USOE are from researchers working outside of colleges of education.

some people believe, for a more effective division of labor. But as yet, there is little coordination among researchers from various disciplines--and especially between people who have been closely identified with Education and those in the basic disciplines (Sieber, 1968; Sieber and Lazarsfeld, 1966).

1. Are problems and experiences in educational research analogous to those associated with the scientific discipline?

People closely identified with educational research sometimes appear to think of this field as a scientific discipline. None of the persons interviewed, however, shared this view. They had difficulty thinking in the framework of analogies with the other sciences. Cole felt that it would be very difficult to use the history of the hard sciences to draw up a program for educational research.

One hazard in trying to draw analogies with the scientific disciplines was cited by Kaplan: that the more mature sciences have grown up in different historical contexts.

> Look at the emergence of a new field in the last 19th or early 20th century and try to compare it with education today. You have to take into account the fundamental fact that the Federal government is pouring huge sums of money into this area now. They want quick results. To compare it to something that happened in the late 19th century just doesn't make sense with this kind of factor operating. When you speak of laissez-faire in the 19th century, you're speaking of it in a laissez-faire context. But you're not talking about laissez-faire in a laissez-faire context today.

Zuckerman suggests that the differences in subject matter between physical and social sciences may present an important barrier against generalizing from one field to the other:

> It's not only a difference in historical location that makes for difficulties in generalizing from physical and biological sciences to social science. The character of the subject matters of each makes any analogies you would draw very loose...At best, you might use a general orientation about the way things are in science. For example, we know that an interdisciplinary approach to research, under the best conditions, is apt to generate problems. So one shouldn't worry too much if such an approach doesn't work out very well in educational research. Moreover, it probably is not wise to spend $100 million trying to foster it. Beyond this simple rule of thumb, I don't think generalizing from one set of disciplines to another

pays off. It is probably a little optimistic to think that all you need to do is to imitate the physical and biological sciences and then all will be well. These sciences have had their troubles too in planning for first-rate research.

But the major difference, as preceived by nearly everyone interviewed, involves the distinction between a scientific discipline and a field of study. "Educational research" is a research field in which the interests of people from several disciplines overlap with those of people from "Education" itself. The problems of a research field are not necessarily comparable to those of science per se. Rather, they are problems which are peculiar to research in the environment of a profession and professional schools. Parallels to the problems of educational research are more likely to be found in the experience of such professions as public health, social work, speech, nursing, business, and engineering than in problems of the disciplines. *

Yet the distinction between disciplines and research fields cannot be applied wholesale to the social sciences. The latter seem to be more open ended and to include a wider range of activities and structures than are typically found in the natural sciences. Ben-David observes that mathematics, for example, is an unambiguous discipline with clearcut boundaries. But in the social sciences (outside economics) there is no common denominator of a central body of highly articulated theory and no clearcut methodology for research. There is only a tradition according to which a set of questions and categories of observation are held in common. Ben-David contends that many fields are "genuinely nondisciplinary" because their problem is not defined intellectually, but rather pragmatically; a great deal of art is involved in solving the problem with existing systematic knowledge.

* The social system of Research is wider in scope than of Science and would include researchers, practitioners, funding agencies, government officials, the professions, universities and the public. "Science" on the other hand--or more specifically, "basic science"--can be viewed as an activity of a much more closed system., i.e., the "scientific community" (Hagstrom, 1965, 1967). As such, the social control agents and reward systems differ, factors which are commented upon throughout the manuscript. For this reason we advocate a distinction between the Sociology of Science and the Sociology of Research. The latter area of study is not an institutional area, but is comprised of the overlap between scientific disciplines, and the groups mentioned above. It is a hybrid area of study, having components of the Sociology of Occupations, Organizational Sociology, and the Sociology of Education, as well as the Sociology of Science.

In short, researchers representing a variety of disciplinary traditions, as well as some people who are themselves graduates of colleges of education, may share a mutual interest in certain substantive problems, but they tend to be concerned with different intellectual problems. Since the boundaries of a discipline are defined by the intellectual framework, "educational research" does not constitute a discipline. However, since the social sciences have not yet matured as disciplines, and hence have only loosely defined boundaries, a greater degree of collaboration may occur than if they were completely mature. This is possible because of greater overlap among the intellectual frameworks and because mutually perceived practical problems are likely to exert greater influence on the direction of the research (about which more will be said later). This raises a second question:

2. <u>How productive would greater cooperation among the disciplines be and how can cooperation be promoted?</u>

One's view of the consequences of interdisciplinary work would seem to depend on whether one is assessing the impact of this work on a <u>science</u> (or the evolution of a new scientific field) or on the development of <u>technology</u> (or improvement of practice). Interdisciplinary work seems more likely to benefit the latter. The term "interdisciplinary" itself in some cases seems to mean theoretical synthesis of different disciplines (e.g. biochemistry). In other cases it refers to only loosely coordinated attacks on similar problems by people with varied academic background. In some instances the term "multidisciplinary" is more appropriate, as when researchers represent several disciplines and do research in a field completely independent of one another. Clark draws a sharp distinction between interdisciplinary work which is basic and that which is applied in nature. Examples of the former include efforts to integrate theoretically coherent areas of overlap between separate disciplines such as biochemistry and physiology, or in the social sciences perhaps the human relations movement. But as Clark notes, although applied research is often interdisciplinary in nature, new hybrid disciplines do not as a rule result from <u>applied</u> collaborative efforts.

If it can be assumed that neither education itself nor educational research represents a discipline, and given the tendency for interdisciplinary work in education (where it occurs) to take an applied bent (in Clark's terms), it does not appear very likely that a hybrid like biochemistry can provide a model for the development of a parallel "science of education" within educational research. In any event, it is important to recognize that biochemistry was the product of efforts to systematize areas of <u>theoretical</u> overlap between what originally were integral disciplines, and it was not necessarily the outcome of collaboration around a particular substantive interest. Ben-David has discussed this type of hybridization (Ben-David, 1960; 1966).

Kuhn observes that most research is conducted within disciplines, and where collaboration does take place, it is likely to be sustained by the formation of a new subdiscipline (such as molecular biology). Lazarsfeld believes that interdisciplinary development occurs within people, that is to say, a new generation. Barber feels that disciplines are forced to evolve new specialized fields as a means of circumventing obsolence. Zuckerman is not, however, optimistic about hybridization developing:

> My own guess is that only very rarely does effective interdisciplinary research result from collaboration between people trained in different disciplines. Rather, a kind of new hybrid has to develop that is separate from one field or the other. Instead of consciously pushing interdisciplinary work, a better way of developing the field (educational research) is to improve the kind of work that is done within each of the separate disciplines, so that you will have better sociology of education, better psychological work done in education, better anthropology and so on. It is unlikely that more cooperation can be fostered in this subject area than has been achieved in the past in any other subject area.

Cole elaborates:

> In the hard sciences, too, there is very little communication between people in different areas of different fields, even if they are working on similar problems; and there is also general hostility toward people in other fields. There is a kind of ethnocentrism.

Kuhn does not expect that anything very productive will come from interdisciplinary research, which he believes is almost a contradiction in terms, since he maintains that most research is done within the context of a discipline. Thinking specifically of the educationist's role in research, though, he does believe that a consultant can play an important part.

> ...I am distinguishing between, on the one hand, calling in consultants from other disciplines to help design solutions to an immediate practical problem and, on the other hand, calling in people from different disciplines and saying, 'Do research together on this series of problems'. The second of these is not likely to be terribly effective.

Ben-David also agrees that the only creative possibilities in interdisciplinary research exist when personnel from various disciplines meet on specific questions. Then communication and loosely coordinated mutual effort is possible and even productive.

Probably too little is known about the general process by which the differentiated parts of a system merge into new mutations to rule out categorically the possibility that an "applied science of education" might evolve out of the common interests of various social scientists and educators studying education. But Clark's comments do indicate that it makes a substantial difference whether the interdisciplinary work is primarily theoretical or primarily applied. This, in turn, depends on how the problems are defined.

One last way of conceptualizing interdisciplinary cooperation should be mentioned here, not only because it is important in its own right, but because it offers a middle road between the highly unlikely "interdisciplinary research" and the somewhat noninstitutionalized "role of consultant". Diana Crane considers the possibility of interdisciplinary communication from the vantage point of her research on scientists who study the diffusion of agricultural innovations. She says:

> A particular research problem is often dealt with by scientists in several disciplines. Some communication and influence on the development of research ideas occur across these specialties, but mainly on the part of the most productive and prestigious members. It may be that, in educational research, people don't develop enough prestige that researchers in other closely related fields will want to communicate with them and read their work. They themselves may not be aware of researchers who are tackling closely related problems in other disciplines. Bringing them together at conferences might be stimulating, especially for the educational researchers.

It is fair to say that to date there has been little interdisciplinary collaboration of any kind in this field. Some of the reasons behind the lack of cooperation among various disciplines have been alluded to; but there may be still other reasons to account for the reluctance of social scientists to work closely with colleagues in colleges of education. One line of explanation places a great deal of stress on status problems, and in particular on invidious distinctions about prestige.

3. <u>Is the lack of cooperation among various types of educational researchers primarily due to prestige distinction?</u>

Kuhn suggests that prestige consideration within universities may have prompted some schools of education to become associated with research. He himself is not certain whether it is valuable even to maintain separate schools of education, or, if indeed they are useful, whether educational research should go on within such institutions. Yet, schools of education, he feels, want to be identified with research, even though in most of them research does not yet have high priority. Conversely, research development in colleges of education, as well as in most other professional schools has been impeded by the apparent [illegible] of researchers in the basic disciplines to become too c[illegible]ntified with the professions themselves. It may some[illegible] that social scientists avoid cooperating with their count[illegible] in colleges of education, and in other disciplines as well, [illegible] order to avoid contaminating the relative prestige o[illegible]iscipline. It is true that scientific prestige is contr[illegible] the disciplines and that the prestige of most professions [illegible] than that of most sciences, at least in the view of many scien[illegible] but it does not seem to us that the prestige of education [illegible]ession, that is, the <u>subject matter per se</u>, accounts fo[illegible]stige of educational research as a field. For, as Laza[illegible]serves:

> I don't think it is now less [illegible]ous to be interested in the sociology of educatio[illegible] less prestigious to be hired by a school of ed[illegible] but that is very different. The difference is [illegible] prestige of the institutions and not in the [illegible] the subject. How to upgrade the prestige of th[illegible] of education is a difficult question.

Another reason why cooperatio[illegible] blocked at this level develops from the fact that the di[illegible] tends to control the reward structure, and hence resear[illegible] loathe to deviate into a peripheral area. (Hagstrom, 196[illegible].) This matter was discussed by the interviewees and [illegible] considered later in the paper.

In general, the answer to the [illegible] why social scientists do not cooperate with colleagues [illegible]on, seems to have as much to do with the structure of scien[illegible] nature of schools of education as with the prestige of [illegible] <u>per se</u>. Hence, we shall consider the prevailing condition[illegible]arch within schools of education.

4. <u>For what reasons are scho[illegible]ucation not highly attractive to social scien[illegible]</u>

Many reasons have been given [illegible] failure of professional schools in general to attract soc[illegible] researchers; these are discussed later in the context [illegible]lishing conditions that might lead social scientists to ch[illegible]r organizational affiliation. Some schools of educatio[illegible]se, are more attractive than others. An important consid[illegible] be the relationship of the school to such outside grou[illegible]mni, with regard to the kind of support given to the [illegible] degree of influence outsiders exert, and the relative [illegible] that is likely to be placed on research, and particula[illegible]ic research. Marcson's observations are pertinent:

> The relatively low prestige [illegible] to educational research possibly results fr[illegible] that schools

> of education have had, and continue to have, low prestige on university campuses. They tend to draw or select people with relatively low prestige. There's another problem as well: a school or college of education is made up of relationships with a number of disciplines, not just one. But the members of schools of education are in turn tied to a whole community of teachers, commissioners of education, superintendents of schools and principals. And what is their attitude toward research? ...This community is not dominated by concepts of empiricism. It is dominated by philosophies of education--philosophies of education which emphasize philosophical values, educational values, and ethical values.

It is not simply a matter of prestige, then, but a question of the amount of support for research and the amount of autonomy which the environment insures the scientist--i.e., his power to select problems for study and to evaluate and distribute rewards for the work done, and his ability to resist pressure for social action or philosophical reflection.*

It was suggested that perhaps some analogy could be drawn with medicine and medical research, as another form of research conducted in the setting of a profession and a professional school. Several of the experts interviewed doubted, however, that an analogy could be made between medical and educational research settings. They felt that prestige and control patterns in medical research are quite different from those in education, with medical research being more attractive to social scientists.

Marcson, for example, sees no comparison between the prestige of the doctor and that of the teacher, or of their respective training institutions: thus he can draw no analogies. But similarities in the prestige level of engineering schools and schools of education do result, he believes, in some shared difficulties.

Some historical differences between medicine and education are mentioned by Ben-David:

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* We do not, of course, have evidence on the relative importance of prestige and of power in these matters. More evidence is needed, too, on the relationship between the evaluations of a scientist as made by his colleagues within his own discipline and the evaluations by academic people outside of his discipline (e.g. people in education). An analysis of citations would provide one way of approaching this question.

The medical schools have a long university tradition starting from the Middle Ages. Perhaps the model scientist was a doctor. Many basic scientists, physicists, chemists, and whatnot, were practicing doctors for whom science was a hobby. It was very much accepted in schools of medicine that some professors should be outstanding scientists, even before the scientific era in the medical schools began. In education, you do not have this tradition.

But Ben-David notes that it is not merely a question of tradition but one of the relative <u>success</u> of applying science to medicine as opposed to education:

> It is probably true that research, in whatever disciplines, has benefited education much less than it has benefited medicine. Practicing doctors and schools of medicine have good reason to believe that most of science helps them. But educators have less reason to believe it. So the problem is not only a matter of attitude. If an educator asks me, "What can sociology contribute to my practical work?" I think whatever I say will indicate a very modest contribution compared to what a biochemist can say if a doctor asks him the same question about biochemistry.

In summary, then, interdisciplinary work is antipathetic to scientists--or at least to social scientists--partly because various disciplines are at different stages of maturity and face their own unique problems. Specialization has the advantage of insulating scientists from the judgments of those colleagues who are most likely to subscribe to methodologies, interests and objectives different from their own. What has to be recognized is that the same distrust that researchers have of funding agencies and possible domination by them also applies to their relationships with alien professions and disciplines. In Zuckerman's characterization:

> Science is not well organized or well articulated. People are jealous and do not necessarily get along with one another. To expect anything other than this, is to be uncritically optimistic.

It has been pointed out that the whole basis of social control in pure science lies in the exchange of new scientific information for recognition from colleagues who are competent to evaluate one's work (Storer, 1966; Hagstrom, 1965). An interdisciplinary organization such as AERA can facilitate the exchange of information. But it will have difficulty providing competent recognition from specialists in a discipline since--unless a new hybrid evolves--it is the discipline which has the authority to act as the primary arbiter of scientific competence. (Of course, the scientist's interdisciplinary reputation is also likely to be affected by the esteem he holds within his discipline. But little is known about the reverse process--i.e., the reverberations on the scientist's reputation within his discipline that may result from the reputation he has achieved among colleagues in other disciplines.)

THE QUALITY OF EDUCATIONAL RESEARCH

The priority given to quality in the reward system of science has been documented. One study found that quality of output is more significant than quantity in eliciting recognition and that the reward system in physics operates to encourage creative researchers to be highly productive while diverting the energies of the less creative into other channels (Cole and Cole, 1967).* Yet there seems to be a widespread impression that research on education has predominantly been of a low quality. There is some evidence to support that impression. Much of the research has been done by people who have themselves graduated from schools of education and/or are currently located in such settings. And, by various reports, most schools of education do not place special emphasis on training researchers (Seider & Lazarsfeld, 1966; Fattu, 1967). Undoubtedly such assessments help to account for the low status of educational research. Fostering high quality research was designated by Zuckerman as the chief problem facing the field.

1. Can uniform standards of quality be applied to educational research?

Beyond the impressionistic level there does not seem to be a complete consensus on the standards appropriate for evaluating research within a multidisciplinary field. The multidisciplinary character of research fields, representing a variety of methodological strategies and levels of intellectual maturity, is a source of disagreement. The fact that educational research cannot be evaluated apart from the separate disciplines involved is indicative of its status as a research field rather than a discipline. On this point, Clark suggests that:

> Insofar as people who are associated with AERA are doing research of some continuing value, it will be judged as important by one of the social sciences. One would then analyze the contributions of members of AERA in terms of the standards of a particular discipline.

But Orlans takes issue with the assumption that quality can be judged only by the disciplines:

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*Hargins and Hagstrom (1967), however, do note that the prestige of the scientist's doctoral institution as well as the quality of this research influences his organizational status.

> The definition of quality is too much under the control of the university community. It is a self-supporting system. The academic man is the one who pronounces on quality. I'd like a second test of quality, a test that is pertinent to the kind of work you're trying to encourage...There is no reason why one can't have pride in doing practical research...The econometrician will never pass the test of quality for the mathematician, but this doesn't mean there isn't good and bad work in econometrics. It's also very difficult in psychology. The clinician has a completely different set of problems from the academic teacher and the research man.

Glaser is concerned about the debilitating effects of applying multiple standards in interdisciplinary fields. This, he believes, leads either to no results or ones so compromised that they are meaningless. There does seem to be some generally agreed upon standards, however. Glaser asserts that the consequences of the research must provide the final test--perhaps as reflected in the frequency of citations, or in the judged generalizability of the conclusions. The significance of the problem of generalizability of findings is implicit in Clark's comment:

> I don't think that educational research, if one defines it as research dealing in some fashion with the institutions of education, is by definition a low-prestige area of sociology, psychology, economics, or political science. If a piece of research has no interest except to the study of educational institutions, it is likely to be of low prestige, because it is likely to be low in quality in terms of the standards of the discipline *per se*. It will be low in prestige only if it is specific to the area of education. But this is true of many kinds of research. If one does a study which has no implications beyond those reported in the study, it will be considered low in prestige.

Aside from extreme cases, however, this is a complicated test, since generalizability itself is partially a function of the available methodologies and of the way the problems are defined, both of which depend upon the maturity of the discipline.

2. What accounts for the quality level of the research field?

However complex the problems involved in assessing quality, there seems to be general agreement about the factors bearing on the conditions responsible for the quality level of a research field.

Training. One such factor is the technical sophistication and commitment of the researchers. Cole claims that too many educational researchers have inadequate research training and experience. And quality of training is itself a product of more basic factors. Colleges of education traditionally have drawn from a limited pool of talent, i.e., persons originally trained to be teachers (Sieber, 1968). Researchers trained within colleges of education have suffered because of the low priority traditionally given to research as compared with the objective of training practitioners. Ben-David notes that it is difficult to create an atmosphere conducive to intellectual creativity in an institution whose primary function is to train practitioners who will fit into slots in the existing system, and whose student body is, for the most part, not interested in research.

Perhaps these problems are endemic to professional school settings: Crane notes a similar situation in schools of nursing. It is very difficult to establish a clearcut role for the researcher in a school of nursing because people there are trained exclusively for hospital work. The same situation--training for narrow expertise--exists in schools of education.

Barber sees the problem of the quality of training for educational research as a part of a more generic problem of obsolescence which affects all research fields:

> It seems to me that the basic problem is not the newness... of this field or the communication problems...but the fact that a large number of people calling themselves educational researchers are on the whole very poorly trained, both as to theory in the basic sciences and, I take it, as to certain kinds of methods...This problem occurs everywhere to some extent in the social sciences: the problem of obsolescence. The basic need is not to communicate to these people, but somehow to retrain them.

He points out that many people in education were trained during a period when research was less important than it is now, but they are, nevertheless, now under pressure to do research. Structural arrangements, in terms of prestige and other incentives must be provided which will encourage established people to be willing to be retrained, and even more important, will encourage people with new ideas to communicate with those who need retraining. He continues:

> One of the ways in which obsolescence in a field can be circumvented it seems to me, is for a few specialized fields to emerge; that is, for the older field to differentiate out into newer subfields. Very often the subfields are attractive to younger people because they will offer all kinds of opportunities for dissertations, jobs, research funds. So the problem of obsolescence is circumvented in part by retraining and in part simply by leaving behind and establishing a whole new category...I think what has happened in the sociology of education has happened in other fields. In effect, a whole new category is becoming a core, as I judge it now by reading the <u>Sociology of Education</u>. I mean new in a substantive

not in a terminological sense. The sociology of education has always existed, but the new sociology of education is substantially different in terms of theory and methods from the older one; in effect, it's a new subspecialty.

If the answer to obsolescence is retraining, however, it is not clear that it is possible to retrain educators who have been extensively socialized to another role. Also, there are differences of opinion about the type of training which is appropriate, and these often seem to reflect differences of opinion about the importance of basic and applied research (which is discussed subsequently). Clark prescribes different forms of training for researchers who will be working in applied as opposed to basic settings. After considering some of the people from basic disciplines who have done good research in education, Clark concludes that each employed a general disciplinary focus for his research problem, using concepts and methods defined by the standards of his discipline. Basic research on education will not be most productively advanced, he feels, by creating a separate corps of persons who will do full-time research on education isolated from basic disciplines.

However, when looking more specifically at the school of education, Clark recommends training of a more applied nature. Since Ed. D's must be problem solvers, Clark believes that they should acquire the tools of relevant disciplines along with their "professional socialization" in the school of education; their training should be more interdisciplinary than traditional graduate work.

Marcson sees the question of quality as a function of the types of problems studied,* which, he maintains, depends not only on training but also on appropriate structures within which to do research. For example, people should be placed in situations where they can study limited problems. Responding to a question about ways to raise the prestige and quality of educational research, he says:

> The issue is the reorganization or restructuring of schools of education in a way to place these matters in what I consider to be their proper proportion. Don't worry at this moment about the prestige of the field. Some basic organizational questions and some basic restructuring come first. I think that prestige will come...from accomplishment rather than from theorizing...It's not a matter

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* Simpson attributes the rise and decline of areas within sociology to the quality and clarity of conceptual definitions in the area. Using papers read at annual meetings as an index, he found that the Sociology of Education was an expanding area between 1950 and 1959 (Simpson, 1961)

of raising research standards...not a matter of knowing the difference between chi-square and correlation...It's a matter of raising the proper questions. (italics supplied)

The process of retraining, as Marcson seems to be thinking of it, requires a time-consuming evolutionary period during which a new discipline-spanning hybrid matures. But he believes that the emphasis in educational research training should be on the relevancy of problems and questions, not on research sophistication. Emphasis on the latter will lead to what he terms an "exercise in isolation".

Several of the experts implied that the evolution of educational research to full maturity may require several generations. In the early stages, Marcson speculates, research on education will be conducted by people from the relevant social science disciplines. After this first stage, though, people with backgrounds in education who have research abilities will make larger contributions because they will know what questions to raise.

In this context, the training of medical researchers was seen as a possible model for what might evolve in education. Ben-David suggests:

> What is a researcher in clinical medicine? He is a man who tries to do scientific research which may be founded on physiology or biochemistry about ailments connected with a part of the body. Usually he is a medical doctor who after one or two years of practice in a hospital plus some research goes into a research institute to receive further training in technique, very often from basic researchers. Then he goes back to do clinical research for the hospital. The question of course is how to maintain the motivation of such people to do research which is relevant to practice. Here there may be a problem which is not so serious in medicine as it is in teaching. In medicine these people are usually part of medical faculties, which are high prestige faculties. The university hospital is a setting which tries to provide clinical material for research.
>
> The situation in schools of education is much more difficult. If you have a man who wants to do research in a certain field, you have to provide him with a setting wherein he feels that he can do what he wants, that what he does is relevant, and that the environment understands him. It seems to me that for this purpose, there is no appropriate setting today in schools of education. If you don't have this possibility for experimentation, then what is the analogue of clinical research in education? How can you create something like clinical research?

Formal Quality Controls. The level of research in a field may be improved by discouraging the less qualified people. Schools of education typically require that educational practitioners "do some research" to obtain an advanced degree. Perhaps alternative standards for preparing practitioners could be found. In addition, perhaps the generous policies of research funding agencies might be revised to reduce the level of available support. What seems to have been a boon to educational research--the rapid expansion of research funds following in the wake of the 1965 Elementary and Secondary Education Act--may have been a disservice. Cole suggests that funding agencies supporting research on education should cut back their budget rather than give grants to persons presenting inadequate credentials or proposals.*

There is some disagreement on the relative importance of journals as a means of improving the quality of research in a field. Arguing that communication precedes community, Hagstrom maintains that a specialized journal is a necessary condition for the formation of such a community, since a scientist's primary reference group is (partially) composed of those who read his published work. A periodical devoted to a field encourages the scientist to conceive of himself as a new kind of specialist (Hagstrom, 1965, p. 210). Crane also attaches a great deal of importance to journals and suggests that developing journals in which people can express their research interests and publish their work is of greater importance than raising standards of membership in the professional association.

However, there are all kinds of journals and informal circulation networks as well. Journals intended to span a multidisciplinary audience, such as the Administration Science Quarterly or the American Educational Research Journal, may face some difficult odds. Kuhn points out:

> Publishing in journals read primarily by people outside one's field does nothing for one's prestige, one's disciplinary prestige at least. By and large people I know in the sciences avoid it, at least until fairly late in life. You'll get more bright young people, which is probably what you most need, if you enable them to do whatever they're doing as part of their professional work published in journals in their own discipline.

Cole adds:

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* Simpson discovered no relationship between the quality of an area in sociology (i.e., the ratio of papers read at meetings to those published in the ASR) and its rate of expansion. Sociology of education and medical sociology were expanding areas with lowest quality. Both areas have recently received large infusions of federal money (Simpson, 1961).

> The institutional structure is essentially organized by disciplines, and not in interdisciplinary fashion. It would do a young sociologist very little good to publish in an AERA journal, for example, if he were interested in the sociology of education.*

But Zuckerman adds the important consideration:

> It is not only the interdisciplinary character of the journal, but its sponsorship, and the quality demanded. The Proceedings of the Royal Society and the Proceedings of the National Academy of Sciences are interdisciplinary, but they are sponsored by people who demand very high quality. It's not easy to get a paper into print in either journal.

Structure of the field. In addition to such formal conditions of a field as training and quality control, the informal structure of the field itself can have an influence on the level of research produced. This is undoubtedly a fruitful area for exploration. A comment by Zuckerman may illustrate the point:

> We do know something about the social arrangements that apparently make for quality. Among them is a clustering of whatever good people there are in a small number of places. It seems to be the case that more good students are produced when you have a few good professors in a place rather than just one. The notion of spreading the wealth apparently is not effective in producing good researchers.**

3. What role do eminent researchers play in the development of research fields?

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* However, to the extent that recognition comes specifically from the specialty rather than from the discipline as a whole (Hagstrom, p. 176), an organization such as AERA with a specialized journal may become important in the researcher's reward system. Thus, economists of education and sociologists of education may begin to exchange information and rewards in a specialty such as "human costs of mandatory schooling"--a specialty which may find warm acceptance in the organization of AERA and in its journal. Also, informal networks could be deliberately supported by AERA.

**The notion of a critical mass is relevant to the recruitment of good junior men and graduate students, who are likely to receive superior training when they have access to a number of good professors. In addition, being in such places puts students and scientists in a favorable position in the communication network and hence increases their visibility (Crance, 1965; Zuckerman, 1967).

Active elite groups are known to play an important role in the process of social change generally. It might be suggested that an obvious way to improve the quality of research in a field is to encourage eminent people from the disciplines to turn their attention to it. Orlans remarks that there is no better way to encourage young people to do research in a certain field than to have the leaders of a discipline exhibit an interest in it.

For the long-run development of the field, however, it seems doubtful that the part-time efforts of eminent men could have more influence than the full commitment of highly motivated young people. Orlans qualified his statement, noting that efforts should not be confined to recruiting the eminent mature people--younger men who do not yet have a career can be much more open minded. Even more emphatically, Ben-David considers it foolish to appeal to the expensive entrenched men when younger people, who are less proud, but qualified to do research, will come of their own free will. It is also advisable to bear in mind what a shift in research fields would entail.

4. <u>Under what conditions do scientists shift their research fields?</u>

There are several dimensions to this problem. One is the amount of change made by a scientist in the scope and generalizability of the research problem investigated, e.g., a change from basic to more applied research. A change in research interests within a discipline represents a second consideration. The third is a change in organizational affiliation within university and/or between professional groups. Each of these possible shifts will be reviewed in turn.

<u>Change in the scope of research interests</u>. Generally speaking, the broader the context in which a problem is defined the easier it will be for people to shift their attention to that problem. Hence, it can be expected that researchers will more readily undertake basic research which concerns variables of relevance to education than research which confines them to more narrow applied problems. It is equally unlikely that they will make long-term commitments to problems that are confined to a particular setting.

The reasons relate to the nature of science as a generalizing process. Because the eminence of a scientist tends to be directly associated with the scope of his theory, the best minds are not likely to be interested in working on limited problems for long. As Barber points out:

> You can't be a "super-star" unless you do basic research... One of the factors which determines the popularity and prestige of a field of research is--I think there's no question about it--the degree of basic-ness. By basicness we mean the extent to which it has a kind of abstract,

systematic, comprehensive quality. When we speak of a theory, we are concerned with how abstract, how systematic, how comprehensive it is as a model for dealing with certain kinds of problems. The situation exists in every field. High quality people prefer to work on basic research.

Shifts in substantive and theoretical research interests. Several distinctions are buried in the simple phrase "change in research interests." It may refer to a change of discipline or to a shift in theoretical focus or in substantive research interests within a discipline. A change in disciplines (e.g. from sociology to economics) requires the scientist to master what is likely to be an entirely new body of literature--new concepts, new theories, and perhaps new methodologies. However, if we assume that neither education nor educational research is a discipline, this prospect does not appear to be of much relevance in this context.

Making a simple modification in substantive research interests within a discipline would not seem to present formidable problems so long as it can be handled within the same theoretical framework of that discipline (e.g. the use of cognitive dissonance theory to study the careers of teachers rather than of nurses).* Crane points out that psychologists are recognizing aspects of education as psychology, and studying them as such.

In any research field, there are specific problems which, with some redefinition can be made more or less relevant to a large variety of more generic issues within the discipline itself. Paraphrasing Hans Zetterberg, Menzel notes:

> The sociologist is sometimes called upon to make a contribution to a problem or a particular application; for example, to help with a police problem of some kind, or medical education, or selling. The mistake that is usually made is to think, "As a sociologist, I am supposed to say something that will help police work, so I should look for what sociologists have said about police work in the past." What the sociologists should do is recognize in the problem of the police the generic problems of an organization, or of any attempt at social control, or any of the problems associated with a bureaucratic-professional line of interaction, and draw on research or theoretical knowledge that has to do with, for example, organizations or professions or bureaucracies, rather than police.

Referring to this particular strategy, Clark concludes:

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* A change in theoretical problem area within a discipline may be more difficult (e.g. a change from cognitive dissonance to exchange theory).

> There is fundamentally no distinction between educational institutions and other institutions which social scientists may investigate...I would say basic research is best evaluated by the standards and criteria of the particular discipline. The best basic research dealing with education is perhaps that which is not specific to education at all, but research which simultaneously has implications for other substantive areas which are investigated by psychologists, sociologists, economists, or political scientists.

Of course, even a simple shift of substantive focus within a discipline may require the researcher to establish new networks of personal relationships and perhaps even new reference groups. The experts cited several factors that prevent people from switching areas readily. There is the problem of breaking into a new informal network of colleagues, so that one can be aware of work before it is published; and developing the contacts necessary to gain access to data and sources of funds.

Why, then, do people shift? There was general agreement that persuasion and vague appeals to "come on over" are not very effective. Rather, the best minds are likely to respond to the challenge of specific problems. Zuckerman feels that people will move over to education only when they find that it is where all the action is. Accordingly, Ben-David claims that the future of basic research in education will depend primarily upon the state of the art of the basic field. Arousing interest, he believes, will not be a problem, because education already represents an important social problem and an increasing social investment.

But if a field's popularity depends in part on the presence of intriguing and significant problems, it seems equally true that its fate will be subject to the vacillating interests of scientists. The interest of many social scientists in educational problems has been a one-shot affair rather than a long-range career shift. Moreover, the definition of a problem represents only one side of the coin--the <u>pull</u> factor. There is also a <u>push</u> factor: people change fields when their own becomes so overcrowded and competitive that it is difficult to achieve recognition and other rewards.* Ben-David observes that the nineteenth century academic scene was conducive to transfers of field and academic departments. The

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* A similar process may contribute to the growth of applied research. That is, as the <u>discipline</u> expands, people establish relationships outside of the discipline and undertake unconventional research in less crowded problem areas. According to Hagstrom, "Competitive pressures assure that less popular areas of research will not be neglected and thereby facilitate the allocation of work in science."

(Hagstrom, 1965, p. 1)

rigidly demarcated departmental system on which naturalistic physiology had been organized was responsible for scholarly advance; yet the opportunities for advancement and recognition were far greater in philosophy departments. The situation fostered role hybridization. Those who transferred carved out a new role--psychology --as a means of reconciling the ensuing identity crises (Ben-David, 1966).

Other factors influencing shifts in fields are the availability of funding and the field's prestige. The exact role of prestige is still indeterminate. Hagstrom hypothesizes that when scientists shift problems within a discipline, they move from areas of low prestige to areas of higher prestige.* But he notes that, "This would be true only for scientists without established reputations. Distinguished scientists may be able to change specialties and carry their prestige with them" (Hagstrom, 1965, pp. 67-68). Shifts between disciplines is another matter. In this case, they are likely to shift from the higher prestige to a lower prestige discipline. Crane argues that researchers leave their own field when it begins to become moribund in comparison to the promise of another field.

Change of Organizational Affiliation. Still other reasons may govern an individual's change of organizational affiliation. Given the fact that scientific disciplines are organized around academic departments, which control the primary scientific rewards, professional schools face a competitive handicap in recruiting scientists. Moreover, for reasons already discussed, schools of education do not reward basic research to the extent that academic departments do. Since professional schools are more closely tied to practitioners than are academic departments, technological development that promises to be of some assistance to practitioners tends to receive higher priority within the professional schools than in the academic departments. The academic community fears that it might lose control over scientists whose primary commiments are to a professional school.

Clark distinguishes between the professional incentives-- such as granting arrangements and institutional support, that permit the researcher to make basic contributions to his discipline while focusing on educational institutions--and the personal values of the researcher, such as his concern for the poor quality of many schools and colleges in America. Crane also stresses monetary incentives. Her comments, however, suggest that schools of education not only fail to control scientific rewards, but are unable to provide the kinds of rewards which industry has utilized so effectively. She points out:

* This may be one reason why appointments outside of departments are not highly esteemed by colleagues.

The basic researcher in an industrial setting is motivated to identify with the applied community because of the financial rewards involved. He can make much more in industry than he can by being a professor at a university. The school of education probably cannot afford to pay him more for his services than he could make in a department of sociology, and so he'd rather be in a department of sociology and get the rewards for being a member of the discipline, a kind of psychological reward.

Consequently, scientists are often reluctant to work in schools of education. These problems currently are aggrevated by the labor market. Efforts of schools of education to attract sociologists are thwarted in part by the fact that there is not a sufficient number of sociologists available even to staff sociology departments.

On the other hand, these problems do not necessarily preclude research in applied educational settings. It is easier to attract researchers when research in a field is considered to be an integral part of a discipline. The fact that studies of diffusion of agricultural innovations, for example, gained stature within sociology may help to account for the willingness of sociologists to accept appointments in institutes and colleges of agriculture.

Moreover, Ben-David has noted that new sciences, such as bacteriology, psychoanalysis and experimental psychology, originated from a concern about practical problems in academically marginal situations on the part of people who eventually evolved hybrid roles (Ben-David, 1960; 1966). And Gordon also concluded from his evaluation of the research in the social psychology of illness that the most innovative research occurred in academically marginal situations (Gordon, 1966). While close administration over research did not seem to be conducive to innovation, neither did complete freedom by itself. Rather, he felt that freedom leads to innovation only when there is an incentive to innovate and when the research consequences are visible to those who will use the research. These conditions can be created by exposing the researcher to practical settings. (Joint appointments with schools of education may not, however, represent the optimal marginal situation; autonomous research institutes offer another alternative.)

Strauss and Glaser stress the advantageous working conditions which professional schools can offer to researchers. Informal discussions with the professional practitioners in the school can help the researcher define problems and interpret data. The researcher's affiliation with a professional school may make it easier to gain access to data, and research is less rigidly bound by the orthodoxies of method and theory of his own disciplines.

From their own experience in initiating a graduate program in sociology within a school of nursing, Glaser and Strauss list several prerequisites for developing research in similar environments The conditions for research set by the professional school are, o ~ urse, important, as is the concept of critical mass: more than one researcher must be hired for a discipline to be effective. Also, the schools of education that have been most successful in recruiting behavioral scientists have been in the prestigious universities. Perhaps these universities have been more supportive of research, and of educational research in particular.

It is essential that the school provide freedom and flexibility for the researcher, encouraging him to pursue his own kind of studies without pressure for application to specific problems in some master plan. Glaser and Strauss feel that it is possible for the professional school to offer even <u>more</u> freedom than a standard academic department, in terms of more flexible time schedules and work relationships, lower teaching loads, and fewer administrative responsibilities. Orlans also feels that the only way to attract good people is to give them security and the freedom to do what they want.

An important incentive that colleges of education could provide for the social scientist is an opportunity to exert an <u>influence</u> on education. For, while influence presumably is not an instrumental component of the reward system of science <u>per se</u> (Storer, 1966,) it does seem to be an important objective of many scientists, and impressionistically at least, a high correlation appears to exist between the prestige of scientists and their positions on influential national committees. Yet social scientists often find their ideas ignored and their suggestions rebuffed by the very people in education who sought them out. Perhaps social scientists would find colleges of education more attractive if they were assured of having real authority in such settings. Marcson believes, however, that if the social scientist develops influence on the basis of his personal relationships and if his work has merit, he will in turn attain authority.

Of course, working conditions are not the entire problem. Eminent men must face the possible resistance of established persons in the professional schools whose traditional influence they threaten. Requiring social scientists to work on narrowly defined applied problems may make them more acceptable to the old guard, but may well discourage the more competent scientists. Perhaps it must be concluded that so-called best minds are not the best source of supply for highly applied work. More suitable labor markets may exist in the terminal M.A. candidates in the basic disciplines.

<u>Shifts in Professional Affiliation</u>. Finally, a shift in research interests may be accompanied by a change in professional associations. However, this frequently does not occur. The number of sociologists doing good research on education, as one example, is greater than the number holding membership in the American Educational Research Association, or even in the Sociology of Education

Section of the American Sociological Association. Ben-David wonders if it makes any difference whether studies of the social context of education are done by people in education or people in sociology; and whether these people consider themselves to be affiliated with the Sociology of Education Section of the ASA or not. What is important is the way in which the research is used and the long-term development of the field itself.

Professional associations within one's own discipline are able to rely on the reinforcement of intellectual, prestige, and influence systems in a way that an association comprised of members of different disciplines can not. The latter can, however, provide a channel for communication among people with similar substantive interests, and might be able to represent people who have similar vested interests. Orlans believes that the AERA can perform an important role in this respect:

> I'd like to see more professional groups like AERA in other areas of activity--like in medical research. Information is very fragmented. Snips of news of a National Science Foundation policy appear in Physics Today or in Chemical and Engineering News. Most of the social science professions have gone to special publications such as American Psychologist and American Sociologist, but those really deal with professional matters...It doesn't add up to a continuing examination of major government programs and significant changes, let alone informing people about them in time to express their views before a change in research policy occurs. AERA has been in a natural position to do that. More efforts of this kind would be to the benefit of the social science professions, and would keep government people on their toes.

Lazarsfeld noted, incidentally, that although professional associations in the United States have not played an important role in research planning, in Europe they do play a major role.

But the kind of reinforcement that comes from similarities in the theoretical interests of the membership and from being in a position to bestow professional esteem are not present in interdisciplinary organizations. The fact is that members of different disciplines are not peers in an intellectual sense: they are not technically competent to judge one another's work with the rigor and credibility of peers within their own disciplines. Clark notes that while the AERA is a professional association in the sense that it brings together persons from several disciplinary backgrounds who have a common substantive interest, it is not a professional association in the sense of having a coherent intellectual framework.

Moreover, since researchers look to professional organizations for support and reinforcement of their objectives, the problem of interdisciplinary differences is compounded when people with widely divergent ideas about the importance of research and research priorities are included in the membership of the same organization. Zuckerman feels that the perspectives of researchers and practitioners are sufficiently different that having them together in the same organization, for other than simply social purposes, doesn't make sense as a way of improving research.

In the final analysis, is it fruitful to encourage people in basic disciplines to change their organizational affiliations, or, for that matter, to shift their substantive research interests? The fundamental problem here, as Kuhn has pointed out elsewhere (Kuhn, 1962), is not converting individuals from one field to another. Rather, the problem which should be addressed is:

5. <u>How do research fields transform themselves?</u>

Encouraging a few people, eminent or newly committed, to devote more time to problems relevant to a field is not an answer. In the first place, eminent people are not likely to enter a low prestige field and, even if induced to do so, it would be unlikely that their presence in itself would upgrade it. Neither, Kuhn believes, would importing sophisticated new methodologies upgrade the field, as these should arise in response to substantive achievements, the kind of achievements that attract good people. Kuhn concluded that educational research can be best accomplished by men staying within their own disciplines but working on those aspects which they see as having a relevance and utility for the problem of education.

What seems to be required as a prerequisite to transforming a field is the creation of new communities of researchers <u>within</u> each discipline who are interested in the same area. A minimum necessity is a "critical mass"--i.e., a small, closely knit nucleus of people dedicated to the field and in a position to train a new generation of graduate students. Their function is not simply to give visibility to a field. They can be effective only if they are in a position actually to influence the field itself--i.e., to help set research priorities, to recruit and train new researchers, and to distribute and evaluate rewards. They must, therefore, have collaboration and support within schools of education. Crane feels that this nucleus of stimulating "high producers" and an "invisible college" may be what schools of education lack. She adds:

> Communication seems to be structured around such people... they are continually being looked to for orientation and guidance. <u>It's not that everybody in the field is in communication with everyone else, but that these people link other members of the field</u>. Each is more or less in communication with his students and with each other. This gives the field a kind of integration. (Italics supplied)

For interdisciplinary communication to occur, people of equivalent high stature from the various fields must be involved.

In summary, the process of evolution of men and fields seems to be built into the structure of science itself, and may not require, or even be responsive to, special efforts to promote it. Fields, as well as men, become obsolete--and if not obsolete, at least overcrowded, and people shift to underdeveloped areas. This process probably accelerates as a discipline matures and disciplinary boundaries crystallize sufficiently to force out marginal persons and those who have dual allegiances.

Encouraging more social scientists to select basic theoretical problems of some potential relevance to education, then does not seem to present a major problem. It can be done without any new interdisciplinary structure, although such a structure might facilitate the process. Persuading social scientists to take appointments in schools of education, or even to communicate regularly with applied researchers in other settings, is a more difficult problem. It will require these schools to compete with academic departments, using the same incentives of freedom for and support of basic research. At the same time, they will have to be able to reconcile basic and applied research interests. Any change which threatens to narrow the scope of the social scientists' research interests, whether a redefinition of the problem or a shift in organizational affiliation, may jeopardize the basis on which he was sought out by the school of education in the first place. So long as schools of education that want persons trained as social scientists continue to neglect to provide the conditions necessary for them to behave as such, social scientists will feel ambivalent towards them.

THE RELATIONSHIP BETWEEN BASIC AND APPLIED SCIENCE

Academic departments and professional schools tend to place different priorities on basic and applied research, development and engineering. The educational research community in general has been under fire to produce more useful knowledge. There are, of course, wide differences of opinion about the meaning and utility of distinctions between applied and basic research.* We will only attempt to summarize them there.

1. <u>To what does the popular distinction between basic and applied refer, and is it a valid one?</u>

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* Kidd (1959) could not find operational criteria which adequately distinguish basic from applied research, and Reagan (1967) suggests abandoning the distinction, but preserving the distinction between "research" and "development". Glaser also find the distinction between basic and applied research idle, as there are implications for both in every research.

Several of the experts interviewed, as well as other writers in sociology, attempted to explicate the difference between the two concepts. In general, each concept refers to (a) a different basis for selecting problems and to (b) different standards for evaluating results. These general differences can be more specifically summarized as follows:

1) theory and technology (improving practice) have different priorities in each type of research.*

Research that is both prompted by a technological problem and evaluated on the basis of its relevance to that problem represents one type of unqualified applied research. However, a problem which is provoked by technology may contribute to theory; and a problem stimulated by theory may contribute to the solution of a technological problem.**

2) Generalizability is a more important criterion for judging the results of basic research, while delimiting the problem and confining its area of applicability are more characteristic of applied work. Delimiting the problem has the effect of limiting the degree of uncertainty and increasing the predictability of obtaining usable results (Gordon, 1966).

In social science, the situation is confounded by the fact that often theory has not advanced to the point where it can be used with any certainty to determine what the crucial problems are and what the fruitful variables are likely to be.

3) Time is a more constraining factor in applied than in basic work.

4) Laymen and potential "users" have more influence in the

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* Carroll (1968) distinguished between basic scientific questions that arise from lack of understanding of some given set of phenomena and applied questions that arise from inability to achieve some practical goal. He points out that frequently the former questions must be answered before the latter can be answered.

**Lazarsfeld, Sewell and Wilensky (1967) make a similar distinction from which they derive four types: (1) Autonomous work without significance; (2) autonomous work with significant contribution to sociology; (3) field induced research without significant findings; (4) field induced work with significant contributions to basic sociological knowledge. However, to assume that research that does not contribute to basic sociological knowledge is without significance is to ignore the relationship of research to technology. It is conceivable that research may contribute to the latter even if it is not theoretically significant.

FIGURE 1

Summary of Difference Between Basic and Applied Research

	Basic Research	Applied Research
Basis for Choice of Problem	Scientist's assessment of the state of the theory	Urgency of a practical problem
Criterion for Evaluating Results	Contribution to theory	Contribution to technology and technical applications
Generalizability of Results	Abstract and comprehensive problems relevant to a wide range of variables	Problems limited to specific settings and issues Relatively predictable results
Time Frame	Relatively unlimited	Sense of urgency and deadline
Social Control	Scientific community controls most aspects of the social system	Scientific community shares control of the social systems with funding agencies and practitioners and users

social system of applied research than in basic research. (See Figure 1)

There is, however, a broad middle category which cannot be handled within the simple dichotomy. "Applied research" tends to be a residual category, in which everything is included that cannot be considered to be "pure" basic research. This source of confusion led many of the experts interviewed to refer to the entire field of educational research as an applied research field, largely on the grounds that the basic knowledge is located within the disciplines. However, much research on education is "applied" only in the sense that at least one variable pertains to education. If the variable was predetermined on that basis, the effect is to delimit the scope of the research in some degree. But it may nevertheless be oriented to the discipline. We shall refer to research in which only one of the major categories of variables has been predetermined by considerations outside of science itself, as "mission-oriented basic research".

Before considering the ways in which basic and applied work might be more effectively integrated, we should consider the advantages that accrue from the fact that, in practice, they often have tended to be insulated from one another in the social sciences.

2. <u>What is gained by insulating basic from applied forms of research?</u>

By legitimating the influence of nonscientific requirements in the research process, applied research may easily contribute to institutional drift and goal displacement within a discipline. Applied work which spans science and technology often must be conducted under more severe constraints than basic work. The researcher faces time pressures and must limit his attention to a few variables which can be easily manipulated within a given setting. Such variables often turn out to be theoretically trivial, and to that extent, the scientist is placed in a poor competitive position with respect to those of his peers who are not so confined by these extrascientific constraints. Also, it seems to be the case that the problems that are the safest and most manageable from the practitioner's point of view are not likely to be the most challenging issues for social scientists; and conversely, the most challenging variables are very often the ones over which the practitioner has the least immediate control. This tendency is reinforced by the psychological bias of the reform establishment.*

There is, however, an anomaly here. For if the researcher does turn his attention to the significant variables, in an applied setting he is likely to find himself identified with one side or another of a politically volatile situation. In doing applied research, the scientist tends to become identified with particular interest groups;

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*A case in point is the tremendous faith which applied researchers and practitioners seem to have that education can be reformed through curriculum development. For example, what appear to be the strongest USOE educational laboratories concentrate on curriculum development rather than on the development of new kinds of schools, more effective ways of relating schools and communities, more effective recruitment of teachers and alternative career patterns for teachers--all of which are more difficult problems, but perhaps ones that are more intriguing to social scientists.

his perspectives may be restricted by the specific demands made by laymen and by time pressures. This may jeopardize the credibility of his results. Some social scientists, in fact, appear to have intentionally used the cloak of science as a front for advancing their own ideological causes. The credibility and integrity of the social science enterprise can be seriously jeopardized when research becomes too closely identified with practical affairs.

Glaser's research suggests another problem. He has found that the motivation of scientists to advance knowledge varies directly with the degree to which colleagues advocate scientific goals. Although their motivation to do basic research was not easily mitigated once it had been established, there was a tendency for the motivation to be inhibited in settings where the scientist worked closely for a time with colleagues in favor of the goal of service (Glaser, 1965).

In another place Kuhn has elaborated on the advantage of insulating science and technology, an advantage more easily provided by academic departments (Kuhn, 1962):

> One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving...Just because he is working only for an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted... Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. In this respect, also, the contrast between natural scientists and many social scientists proves instructive. The latter often tend, as the former almost never do, to defend their choice of a research problem--e.g., the effects of racial discrimination or the causes of the business cycle--chiefly in terms of the social importance of achieving a solution.

He remarks, however:

> There is a barely clear case, though a thoroughly controversial one, to be made for the view that research in contemporary physics is being increasingly distorted by the extensive involvement of physicists in pressing social problems. At the other end of the spectrum, one of the reasons that the biological sciences were as late in developing as they were was their attachment to the medical profession...There had to develop a group of

talented people with medical backgrounds, who were not, in fact, constantly dominated by the need to cure people and could turn their curiosity toward biological organisms whether the nature of their work had bearing on curing people or not...I think there are very serious conflicts between the two, and it seems to me that it will, by and large, not be the case that the same people will work both on the advancement of knowledge and on immediate technological problems.

Given the prospect of the high cost of trying to wed science and technology, is the potential payoff likely to be worth it? Probably most people assume so. And the fact is that the natural sciences seem to have made considerable progress precisely the time when their utility is becoming more widely recognized. But Kuhn nevertheless, questions the value of efforts to bring science to bear on technological problems. Such efforts have only recently been consistently successful, and then only with previously highly developed sciences. While the 17th and 18th centuries saw many attempts to persuade scientists to apply themselves to the solution of pressing social problems,* Kuhn nevertheless believes that, by and large, the sciences that were then most developed did not appear to have much, practical application. He points out that until very recently technology developed almost totally independently of theoretical knowledge. Today's tendency to try to apply existing knowledge to economic and social problems has become a pattern of some importance, Kuhn feels, only since about 1880. For these reasons, he believes that it would be premature to rely upon applied research, although he would suggest using scientists in a consulting capacity:

> A direct research assault on these (social) problems is not likely to be a productive one. The gap between the problems and available theory is simply too large to be resolved by existing research, or by existing research directions, or by any new ones that can be thought of or implemented in time to do good...I am very dubious about whether the answer to a concrete social problem such as "How do we teach ghetto children?" is to call in a sociologist, an educator, a psychologists, etc., and design a research problem. It may very well be advisable to call in that same group of people and get their advice on the policy question...But the sort of research project that might then prove to be relevant probably ought not to be called a research project at all.

Needless to say, this is still a controversial issue.

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*Robert K. Merton, "Science and Economy of Seventeenth Century England," in <u>Social Theory and Social Structure</u>, ed. R. K. Merton (New York: Free Press, 1957), pp. 607-27.

3. <u>What then is the appropriate relationship between basic and applied research?</u>

We have noted some of the potential hazards and uncertain outcomes of achieving closer coordination between basic and applied research. New norms that have evolved within science make it difficult, however, to accept the premise that a scientist's reputation among his peers is based solely on his contribution to knowledge. Scientific communities, like other communities, must come to grips with the outside world, that is, must impart their findings to laymen, and gain their support. There appear to be a correlation, at least at the upper reaches of the status system in science, among scholarly prestige, income, and influence within a discipline, and perhaps influence outside the discipline as well.

Until a discipline has developed a theory mature enough to dictate the direction of research, practical problems may have to act as a substitute means for providing the challenges which are so necessary to stimulate new theoretical points of departure. The relative importance of internal mechanisms within a discipline versus ideas brought in from the outside for scientific advancement probably depends on the maturity of the discipline. Perhaps outside influence plays a less important role in mature disciplines in which there is a clear paradigm during periods that Kuhn refers to as 'normal science'. The existence of a paradigm gives direction to a field. Where it exists, people must be immersed in the discipline and sufficiently oriented to its theory to see anomalies which eventually lead to intellectual revolutions.

Yet, even in relatively advanced sciences, Kuhn also recognizes that the men who have invented the fundamentally new paradigms almost always have been either very young, or very new to their fields. It can be argued that the new problems within a science are most likely to be raised by marginal men who have shifted their disciplines. Work on innovations in science (Menzel, 1960; Ben-David, 1966) and education (Carlson, 1965) points to the importance of marginal men (although these studies appear to be as concerned with technology as with science).

The same mechanisms responsible for releasing creative power when scholars shift their vision <u>within</u> the world of science also seem to act when practitioners confront men of science with their practical problems. Indeed, the less mature disciplines, which do not yet have paradigms, probably must rely on these outside challenges for direction. As Ben-David points out elsewhere, ideas do not simply beget ideas; they grow only when people are interested in them occupationally as well as intellectually. Roles, associations and outside support are important (Ben-David, 1966). As he has noted (1960), the German medical system lost efficiency because it became too remote from practice to be able to reorient its frame of reference to take advantage of such new developments as bacteriology and psychoanalysis.

Barber believes that research can generate intellectual problems that could never have been generated by theory alone. Crane concurs:

> In the history of science there are cases where basic science hasn't helped very much. (Rather) certain scientists happened to be working on applied problems. Some of Pasteur's basic research grew out of an applied interest in fermentation, for example. So it works both ways, one example balancing another. How relevant this is to the whole question of research on social problems, I don't know; and I doubt if anybody at this point knows.

In summary, an empirical science cannot afford to become completely insulated from practice. Structural and cultural lag resulting from the premature closure of a discipline can be as much of a threat to scientific advancement as goal displacement brought about by outside pressures. The contribution which outside challenges can make to scientific advancement is undoubtedly related to the level of the discipline's maturity. Disciplines in what Kuhn refers to as the 'pre-paradigm' stage, which do not have a mature theory, perhaps <u>must</u> depend upon outside challenges to do what the theory cannot--promote intellectual revolutions.

4. <u>How can a more effective intergration between basic and applied research be achieved?</u>

<u>The State of the Field</u>. Successful integration between basic and applied research depends upon several conditions. Basic and applied research can be more profitably coordinated at certain stages in the maturity of a research field than at other stages. It depends to a great extent upon the nature of the problem itself and the degree of progress that has been achieved in arriving at a workable theoretical paradigm. Kaplan emphatically states:

> There is a mistake in the underlying assumption...that you need a Manhattan Project to make an A-bomb for Education. That's what everybody is really talking about; to bring in the money and get the really smart people from all the disciplines and start them working on an A-bomb for Education. ...But we're not at that stage at all. In 1939-40 when the Manhattan project was put together to make an A-bomb it was understood that nuclear fission was possible and that it could make a bomb.

Kaplan goes on to point out that although many technical problems had to be worked out, the essentials were there: the developers knew, on the basis of the theory that was available and the work that had already been done, that an A-bomb was theoretically possible.

<u>A Clear Division of Labor.</u> A prerequisite for coordination is a clearer differentiation among the various roles involved than exists

at present. Blurred boundary lines may facilitate the natural, informal processes of coordination, but planned efforts require a certain amount of clarity about what is to be coordinated, if only in order to provide the appropriate types of support and to justify a degree of specialization. Predicting that roles in educational research will gradually become more differentiated, Barber observes that people who are claiming to be researchers are really inadequate engineers.

Separate Structures. Third, although there is some difference of opinion, basic and applied research would seem to require separate structures. Orlans suggests incorporating applied research into academic departments, which, he advocates, should include faculty members whose interests span the various applied fields as well as the specialized areas of the discipline itself. If the applied researcher, whether psychologist or sociologist, goes instead into a professional school, he will be placed in an inappropriate career line and both he and the school will suffer.

Clark is more insistent about separating the two functions. He believes that research institutes and universities serve distinct functions, a fact which should be kept in mind in allocating resources and personnel to both.* The university, he reminds us, as a basic research and advanced training facility, is especially unsuitable for development work. R and D work, because of scheduling requirements, budgetary demands, and outside claims, requires stronger centralized controls than a university provides.

Cole would like to see applied work put on a frankly profit-making basis (it gets the best results), while basic research is conducted by academically-trained personnel. He observes that one factor in the effectiveness of commercial drug companies in promoting applied research is the profit motivation to convert what science has discovered into technology.

Marcson notes that engineering labs in industrial settings have developed special mechanisms for integrating basic and applied research. Directors of engineering laboratories "do not sit idly by waiting for the fruits of basic research." Rather, they place a few persons with training in basic science into technical settings (and vice versa) where applied research is the main objective; applied researchers dominate and they initiate most of the questions. He continues:

> One has [illegible] keep in mind the purposes and objectives of the engineering school: to make use of the findings of basic

*The advantages of research institutes are discussed by Lazarsfeld (1962).

> disciplines, to reorganize them for applied technology. All important industrial laboratories are organized so that a small group is devoted to basic research...That is, the individuals in such a group conduct research along the lines that they find to be of some interest to themselves. But they don't do that 100 percent of the time. Some fraction of their time they make available as consultants to other members of the laboratory who are in applied work. So if you are not in the basic research part of the laboratory and you have a problem, you know you can go to those men, and you know them well enough to know who will be able to help you most to develop new technology.*

Definition of the Problems. Integrating applied work with basic research is not simply a matter of organization, but also in large part a function of the way the problems are defined. Again Marcson's comments are revealing:

> In television, the camera has to take a picture and transmit it...If you don't have a good picture you won't transmit anything good. That's exactly what the early days of television were like. The problem was put to the physicists: "We've solved all the basic general problems, but we're getting a terrible picture." So the physicists took the problem, and asked themselves "What does the eye do?" This is a question ordinarily asked by physiologists. But the physicists made a study of the functions of the eye, and decided how the eye takes a picture. It takes numerous pictures and not just one. Then the problem was simple--that's what the tube had to do. They had set the prescription and all that was needed was solving the problem of making a tube that would take a number of pictures. Now who was doing basic research there and who was doing applied?

Ben-David also emphasizes the crucial role which the definition of research problems plays in articulating basic and applied research. Referring to John Gardner's indictment of universities for not being sufficiently concerned with practical problems, he comments:

> People who talk like this really do not have any specific

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*However, elsewhere Marcson has noted that there is always a tug-of-war in these settings between basic and applied researchers and development people. The tension is especially apparent in the new recruit's aspirations to do fundamental work relative to the actual prospects for doing so. The applied direction of the research is maintained by management through the reward system and the socialization process in the labs--as in any organizational setting (Marcson, 1960).

> problem in mind...It is not enough to say. "You researchers should really take an interest in our practical problems." Why should we? We are not practical people. If the practical people will come to a researcher and say, "This is my problem: I want to do so and so, I have so much money for it, and I want you to make a specific recommendation whether I should do it one way or another," then I believe they could find the proper people to do this kind of research.

The kinds of questions that are raised by researchers themselves depend upon their own conception of the field--its objectives and its possibilities. Menzel notes that there are strong pressures to move the field of speech, for example, further in the direction of basic research. Although this is essentially a healthy trend, Menzel sees a danger in making basic research contributions a prime criterion in a field like speech. If this field were to define its activities as consisting of basic research, it would, on the one hand, fail to make the applied contributions to fields like broadcasting for which it is uniquely qualified, and, on the other hand, in essence it would simply duplicate what can be done better within the basic discipline.

The propensity of applied researchers to identify too closely with a discipline and to use academic departments as reference groups has, in Marcson's opinion, distracted their attention from the central problems in many fields. He suggests that the tendency of some educational researchers to think of education as a discipline encourages the doubtful assumption that direct questions about learning and teaching can be raised--and answered. In his opinion, many questions that might be raised in education are as inaccessible to answers as were questions in psychology about creativity. As with the subject of creativity, perhaps questions of learning and teaching cannot be attacked directly head on. Cole suggests a middle road of research that may be most promising: evaluation research. Perhaps evaluation can be a highly productive way of defining problems in such a manner as to be relevant to both social science and education.

The Practitioner's Indifference to the Use of Research. Finally, it should be recognized that much of what appears to be the failure of research to produce practical payoff has as much to do first, with the absence of a well articulated division of labor for adapting knowledge and disseminating new developments, and second, the reluctance of laymen to use what research findings are available. As Ben-David and Zuckerman point out, researcher and practitioner must both have an interest before communication between them can arise, and indifference is as common with the layman as with the researcher. Cole makes concrete recommendations that might encourage practitioners to use research:

> I would say, make the practitioner's tenure dependent upon

measurable success in educational people...Researchers should be able to specify what the practitioners should do in order to get a change in measurable output. Then one has to make sure that the practitioner does what the researcher says he should do...If he can improve the quality of education without research, then his methods are better. If he can't, then we should get people who will help kids learn to read better. That's what we're interested in doing...Until we develop measures of the success of the education going on, we'll have neither good educational research nor good education.

Crane notes that, unlike the situation in industrial research, in educational research there are as yet no good economic or other indicators of the usefulness of a product, so that there may be problems in getting an innovation adopted by organizations other than the one which produced it. Perhaps one reason for the lack of innovation in the public schools is the monopolistic nature of the schools: they do not have to compete for students and seemingly they have little "economic" need to better themselves.

Of course, another fundamental reason for the lag in utilization of educational research is that it has not necessarily been demonstrated to be useful. When good scientists neglect development work and fail to appreciate the difference between basic and applied research, many results may be inapplicable. Ben-David elaborates:

> Education, as it is organized today, especially at the elementary and secondary levels, is so tied to rigid regulations and interlocking interests of political pressure groups, teacher associations and state and local governments, that by the time any change is made, one is completely exhausted. This is why the practical administrator is rather skeptical about the possibility of using research. The other problem is, of course, that many research results, even if they are good research, are not applicable. The aim of the researcher was to isolate certain conditions and understand certain effects. What may be true under the conditions the researcher created may not be applicable in practice. Most social science research is of this kind. If you try to make the results into educational policy, you may do a great deal of harm. Few social scientists are fully aware of the difference between basic research and practical application. In science everyone knows that usually a tremendous amount of development work needs to be done before a basic discovery can be turned into a useful product or procedure. In the social sciences, however, we very often only add to our report a superficial paragraph about the application of our results, and some of us believe that this makes those results really applicable.

In summary, underlying the question of the level of priority appropriate to practical problems is the larger question of whether the social sciences are or should be open systems. The answer depends upon both the value attached to the long-run advancement of science in comparison to the (short-run) development of technology, and one's assessment of the utility which outside stimulus has for the development of science itself.

Perhaps at times all that stands in the way of application is a concerted effort at translation and adaptation of results. But perhaps too, there is little cause to believe that basic research will automatically produce the desired technological developments. For this reason, it may be necessary to give technological development separate attention within some national system of priorities for research and development. But then the unanswered question is whether close relationships between science and technology will tend to undermine scientific advancement itself. There is no formula for deciding the degree of closure appropriate for a given stage of scientific maturity (nor perhaps even for assessing that level of maturity); and premature closure may be as detrimental as insufficient autonomy. It would appear to be risky for any science to rely entirely on theory for its direction, and foolhardy for the social sciences to attempt to do so at their present level of development. Applied problems can provide one source of stimulation for basic research. Attempts to reconcile theory and practice via applied research perhaps could jeopardize science, but could also spread some of the burden and some of the risk involved in the development of a science.

CONTROL OVER RESEARCH POLICY

Currently, a tension exists between people who identify themselves as basic researchers interested in education and federal bureaucrats in charge of distributing funds for educational research. Officials who must justify expenditures of research funds to Congress are under pressure to produce visible results. Social scientists are thus called upon to devote more effort to solving social problems in addition to studying their causes.

Officials in charge of research support operations in USOE in recent years have taken the position that, if research is to be valuable, it must be usable, and that the prospective uses should be considered before the project is initiated. The USOE in the last several years has increased its use of solicited proposals, i.e., "procured research," and an ever-increasing amount of money is supporting coordinated attacks on important social problems.

Various members of the research community disagree about the relative productiveness of coordinated efforts compared to the traditional laissez-faire approach to science. One educational researcher has written: "There is little to be gained from the shotgun approach of just searching for new knowledge with the hope that somebody might follow in our footsteps and find some practical use for it" (Goldhammer, 1968).

Taking into account the constraints imposed by the state of the discipline, the norms of the research community, and the supply of well-trained and creative researcher, the question may be asked:

1. <u>Does deliberate planning by the Federal government accelerate the advance of science or technology?</u>

Clearly, the answer to this question depends on many factors, such as how priorities are defined and what relationships exist between the research community and the government. The problem, however, is generic to all research fields. And in nearly every case, the failure to distinguish between the advancement of science and the advancement of technology undoubtedly has been responsible for much of the confusion over the issue.

Kuhn believes that during what he calls the pre-paradigm period of science, there is no possibility whatever of planning for <u>basic research</u> in such a way as to accelerate the construction of theories powerful enough to guide a frontal assault on a particular problem. And once the paradigm stage has been reached, the paradigm itself will structure the field, so that it is not necessary for outside groups to do so. Speaking specifically of the educational research field, Kuhn continues:

> I'm not sure that there can now be such a thing as really productive educational research. It is not clear that one yet has the conceptual research categories, research tools, properly selected problems that wil lead to increased understanding of the educational proc s. There is a general assumption that if you've got a bi roblem, the way to solve it is by the application of s ence. All you have to do is call on the right people and ut enough money in and in a matter of a few years, you wi have it. But it doesn't work that way, and it never will.

Kaplan also has doubts:

> I'm not sure that getting people to work on what looks like the most necessary applied problems will extend your knowledge in the field very much. Take all the people doing cancer research. Some of those people are, as plain as can be, looking for a cure for cancer. Others do not seem at all concerned with a cure. They're interested in problems of cell growth and cell degeneration, all definitely related to cancer, some more, some less. But nobody can predict at this time where that cure will actually come from: whether from a man who says he's working for a cure or from someone who says he's not really interested in cancer as such. It seems to me there really is no hard evidence one way or another.

Kaplan also points out that the advisability of coordination depends upon the particular stage of knowledge of the discipline. If coordination is attempted too early, one may be coordinating nonsense.

Neither Strauss nor Glaser favors an overall policy dictating research priorities. Glaser does suggest, however, that after a substantial amount of research has been done, a study could be made which synthesizes it. One might attempt to 'draw out' the major theoretical import and major application.

Lazarsfeld, on the other hand, argues for a certain amount of coordination and guidance, since the number of good people in a field is always small. Marcson, agrees, maintaining that in the case of the social sciences, with a small volume of research and a large range of problems to be studied, social scientists are as likely to study the election process, for example, as they are to study education, unless given some guidance and support for specified types of problems.

Though we find no clearcut answer to the issue, there does seem to be some sentiment in favor of planning for applied research after a field has reached the point at which it seems reasonably certain that the desired results can be achieved. There is less agreement about the authority and responsibility of the Federal government to do the planning:

2. <u>What is the proper role of the Federal government in establishing research priorities?</u>

Although it seems to be generally assumed that the Federal government's policies do in fact influence the direction of research, Ben-David asks: "Does the direction of research actually follow the funds; or does the availability of funds in a field reflect the fact that the field has developed to a point where it can promise payoff?" He explains:

> It has often been argued that in the natural sciences today research is very much oriented towards problems of military relevance, because this is where the government puts money. I am somewhat doubtful about it. For instance, the physics on which all the uses of thermonuclear energy are based was the purest of the pure physics, at least until 1939...In fact, wherever there is some kind of indication that a socially important discovery can be made, the money is forthcoming.

Whether the government should make the determination remains a moot point. It seems clear, though, that as the investment in a field of research becomes a larger and a more visible component of the national budget, the government will make a greater effort to control its direction, and this shift of governmental policy is always likely to be a source of discontent for people who benefited from less control. Orlans seems reconciled to the fact that there will be perpetual discontent with whatever priorities are set:

> In general, there is a constant battle in every agency for money, and certainly the academic community is not above this battle. Every discipline wants more money, and within each discipline each faction wants more for itself. I would say that as budgets go up, it is easier to satisfy more varied pressures; and if the budget is stabilized, then other choices have to be made and there are more unhappy people. By and large, the government has a stronger voice in the more applied programs than in the basic ones. One problem right now, is that the Office of Education is trying to move from a background of work that was basic to one that is more applied. They're trying to hold onto more strings, at least in the applied field. I think this is a very common pattern in Washington.

But if it is inevitable that the government will attempt to exert more control as research budgets stabilize, it is also true that in some fields of science and in some agencies, the research community--or rather some members of it--has been an important counterforce.

3. <u>In what ways have researchers been able to influence government priorities for research?</u>

In the physical sciences, the agencies distributing research funds frequently are headed and staffed by leading scientists. Zuckerman speculates that the fact that a first class physical chemist heads the Atomic Energy Commission perhaps prevents a split from developing between bureaucrats and professionals. She notes that it is a common practice to appoint professionals to such agencies on a temporary basis.

However, the route by which physical scientists have come into these positions of influence within operating agencies is less obvious. Many factors appear to be involved. For one thing, physical scientists are less reluctant than social scientists to accept employment outside the academy. (This may reflect the greater security provided by more mature disciplines.) Physical scientists were in on the ground floor when many of the newer agencies developed. (This is not the case for an old line agency like the Office of Education.) Research in the physical sciences seems to have demonstrated far higher payoffs for technology; although the actual evidence of this is not impressive (Sherwin and Isenson, 1966). And, the physical science disciplines are too advanced for laymen to pretend to understand them. On this last point, Cole observes that few laymen would try to tell physicists what to do, whereas many people in education without research backgrounds feel that they are social scientists.

Educational researchers, then, and social scientists in general, are likely to continue to have conflicts with government agencies over the control of research priorities. In addition, there will be growing alarm about the slowdown in the rate of growth of funds; the size of the research kitty now makes it more worth fighting over. And an important factor lies in the background. A large amount of funding being authorized under legislation for "research" is in fact intended for use in demonstration and service activities. Just as it was once necessary for research to be smuggled in under the guise of service, now a variety of activities are being conducted under the rubric of research. The fact that research has become so closely tied to application has served to identify it with special interests sponsoring given changes. This has jeopardized the credibility of all research conducted within operational settings.

Perhaps one way of reducing some of the tension between researchers and federal bureaucrats who tend to set priorities in favor of applied work would be to distinguish clearly the purpose of the funds through separate legislation for basic applied research, development, etc., and allocate a fixed percentage of all funds for education to each research function. Then, the researcher would have recourse through the legislative process.

SUMMARY AND CONCLUSIONS

There was general agreement that educational research represents a social system of wider scope and complexity than the system of a "pure" science. The reward system and goals, and hence the direction of the field, are influenced not only by the norms and interests of several cometing disciplines, but also by the interests of practitioners, funding agencies and government officials. Accordingly, the problems of the field are not so much the problem of science per se as they are the problems of doing scientific research within the context of professional schools and, more generally, of an independent, institutionalized sphere of activity, i.e., a substantive field. The closer the scientist's work environment to the practitioner's, the more he is likely to be subject to the technological basis of evaluation. At the same time, the less well developed the theory of the scientist's discipline (to the extent that he recognizes its inadequacies), the more reliance he must place upon practical problems to stimulate and guide his work.

At the present time, the field of educational research is fragmented along two or three primary patterns. One pattern involves basic researchers from several disciplines who may select to study a substantive educational problem because it coincides with a related intellectual problem of the particular discipline and is subject to the standard methodologies of that discipline. The problems studied tend to be dictated by the degree of generalizability promised by the conclusions and by the state of the discipline itself.

A significant variant of this pattern is represented by researchers originally trained in the basic disciplines, but who have become closely affiliated with educational settings (colleges of education or research institutes) during the course of their careers. In these more marginal settings, the selection of even the basic research problems may be heavily influenced by the closer association of the researcher with practitioners.

The third pattern involves applied researchers whose training has been in colleges of education, who may have had prior experience as educational practitioners, and who tend to begin their research from an interest in a specific substantive educational problem. They may attempt to draw upon a variety of intellectual traditions and methodologies to produce the technology necessary for resolving the problems, but their focus is nondisciplinary, i.e., outside of any particular discipline.

It is possible that these relatively distinct patterns could be synthesized and new hybrid roles created. As yet, this has not occurred.

The problems of educational research are products of the ways the disciplines, professional associations, universities and science itself have been organized. They are the result of an imposing set of structural barriers which stand in the way of scientific objectives. It is forcefully clear that any lasting change will require some reorganization of the institutions involved--the universities, the profession of education and the professional associations and science itself. There are several--not necessarily mutually exclusive--alternatives.

At one extreme, educational research could be promoted entirely within academic department. Several of the authorities interviewed leaned in this direction. Their basic premise seemed to be that science, after all, is structured around the disciplines, which in turn are anchored in the academy. However, to leave the fate of a research field completely to the academic departments is also to subject it entirely to the fortunes of the separate discipline. The attention given by members of a discipline to sub-fields tends to be sporadic and often short lived, depending upon the vacillating objectives of the discipline and its own internal constraints, including competition from other substantive areas for the short supply of social scientists. Consequently, when a researcher from an academic department chooses a substantive educational problem he does not therefore necessarily consider himself to be part of an educational research community, unless such a community already exists on some other basis.

If mission-oriented basic research is to thrive within academic departments, systematic outside support must be provided. The availability of funding for project research is necessary, but it is not sufficient to stabilize the situation. Making more far-reaching research training programs available to the various academic departments would help to promote more coherent identity among educational researchers from each discipline.

But even with such measures, so long as the field remains scattered throughout several disciplines, it will continue to be fragmented unless other provisions are made. As one possibility, a new hybrid discipline could emerge based upon areas of overlapping theory among the various disciplines involved. Thus far, however, the primary overlap among disciplines has been more substantive, and even the substantive interests are quite diffuse; overlap on a topical problem like juvenile delinquency does not in itself lead to groupings around significant *intellectual* problems of the field. In fact, there is little agreement about what the significant intellectual problems are, or how to translate practical problems into intellectual ones. But the possibility that some form of synthesis will take place cannot be ruled out entirely; a handful of brave souls willing to risk breaking with the tradition of their own disciplines could perhaps develop a hybrid role.

As a second and more feasible strategy, closer coordination might be developed among the subspecialties of the various disciplines, for example, around selected *problems*. In this case, too, it would be necessary to maintain key linkages with each of the disciplines in question so that researchers do not become isolated from their disciplines by virtue of their interest in the field of education.

A second distinct alternative is to promote applied research within colleges of education. At present, these colleges are among the few permanent champions of educational research. There is also reason to believe that such settings tend to suggest different intellectual questions to researchers than do settings that are completely insulated from the viewpoints of practitioners.

However, the traditional structure of colleges of education does not appear to be conducive to the development of research. Even researchers connected with colleges of education differ on the significance of the several disciplines involved and are further split on the importance of service, development, applied research and basic research. In addition, research competes with the more central objectives of training.

In order to raise the quality of applied research within colleges of education it will be necessary somehow to modify the colleges themselves. Reorganizing career channels might be one place to start. Lazarsfeld pointed out that better research training by itself would be insufficient unless a more effective career line into educational research were established. Since education graduate students must have prior work experience before enrolling in graduate school, they are more likely to be inclined to enter a training program for practitioners than to prepare for research or other intellectual endeavors. More intensive studies of the efforts made by Harvard and Chicago, for example, to bring various disciplines together would be enlightening, as would the study of the role of social scientists in schools of medicine, public administration, and other professional schools.

Even where it is not possible to reorganize colleges of education, it may be possible to elevate the priority given to research within the existing structure. As a minimum, this would entail the selection of a few influential schools of education to receive a degree of support sufficient to provide the conditions necessary to attract a critical mass of researchers from the relevant disciplines. These conditions include sufficient freedom, opportunity to associate with practitioners as well as disciplinary colleagues, authority within the schools, and better access to data, more flexible schedules, and perhaps better funding for research than academic departments are able to provide.

A third alternative is to remove educational research from the domination of both academic departments and schools of education and support it primarily through research institutes. Most applied work should perhaps be entirely outside of the university. Closer cooperation between educational researchers and commercial enterprises interested in education would assist the diffusion of the results of educational research by making use of the effective diffusion system already present in the publishing industry. (Adequate safeguards would be necessary to prevent premature use of new findings). More thought needs to be given to the most effective ways of organizing the internal structure of an institute for social research. Government and industrial laboratories are organized around problems such as electronics or aerodynamics, rather than parent disciplines. The comparable problems in education are urban education, decentralization and so forth.

Finally, a fourth alternative is to work towards a deliberate reorganization of the field of educational research itself. Several conditions must exist before a field can become fully institutionalized and develop a coherent framework. A self-conscious community must develop; the boundaries of the field must be clarified and provisions made for protecting them; and a systematic division of labor must be devised, together with the necessary structures for integrating the various functions.

A uniform identity. The more broadly its objectives are defined, the wider the base of support the field will have the more adaptable it can remain. But at the same time, the concessions necessary to maintain this broad base of support will constrain people in the field from formulating more explicit objectives and policy statements that could provide leadership.

Coordination. In the relatively closed system of "normal science" there is less need for formalized coordinating mechanisms, since goals, resources, and sanctions are controlled within the scientific community. Because of the complexity and interdependence of the social system of educational research, however, informal mechanisms are not sufficient to promote stability and development. Coordinative and communicative devices are needed.

It was Orlans' suggestion that the American Academy of Sciences might be studied as a model of a prestigious and influential group which is concentrating on high level government policy toward research. Between the National Academy of Education, the American Academy of Science, and the American Educational Research Association, a prototype might be found that would effectively coordinate the interests of people doing research in education.

If administrative articulation between applied and basic research is to be improved, it might be necessary to differentiate more clearly between basic and applied research activities and to provide separate structures for each type. At the same time, achieving an intellectual integration between the applied and the basic depends upon finding meaningful problems. A national structure might be established for the purpose of exploring ways to identify problems that may be compatible with basic and applied researchers as well as non-scientists.

Independent permanent commissions might be set up to work on the long-range policy issues. One function of such commissions would be to establish the intellectual boundaries of the field. Relevant research findings from the various disciplines could be compiled, and their implications drawn for practice. Conceivably this could lead to more systematic synthesis and integration of middle-range theoretical work as well But the primary initial responsibility of such commissions would be to provide a forum for translating practical problems into the key intellectual problems of the field and suggesting applications for work already accomplished.

As a structures are needed to encourage sub-communitie ir ach discipline and with elected colleagues from other discip... These communities must be allowed to develop around similar theoretical interests as well as around similar practical problems and topical concerns. While there seems to be no urgent need to promote large scale cooperation across disciplines, such conditions should be provided which might encourage a few of the most committed researchers from each discipline to work together on mutual concerns.

Third, task forces might be established for the purpose of conducting either basic or applied research on specific problems. Some task forces might cut across disciplines; others might utilize various subspecialties within a discipline; and still other task forces might focus on the problems of the field itself.

In addition, the establishment of more research institutes should be supported. At the same time, social scientists should be encouraged to join schools of education and otherwise to cooperate more closely with professional education. Ways would need to be devised to assure them sufficient autonomy and support for their work. Finally, new forms of linkage between practitioners and scientists might be explored. Perhaps means can be found to allow some laymen to play a more influential role in the research process without violating the integrity of science. For example, they might be systematically included as advisors in certain types of projects at the stages of problem conceptualization, sample selection, and drawing implications from the findings. This is being done in a National Study of Indian Education being conducted by Havighurst (Aurbach, 1967). Studies of lay participation in poverty programs and community involvement in bussing and decentralization plans could prove helpful here.

Allocation of rewards and other resources. Given the multidisciplinary character of the field of educational research, a necessary minimum seems to be that all disciplines concerned be represented on the editorial boards of the journals in the field, and that prospective publications be screened on the basis of the standards of each discipline, using standards relevant for basic and applied research as approximate. Similarly, awards should be decided upon by an interdisciplinary committee or separate awards given by each discipline.

Political power. To protect its boundaries, a field must have political as well as intellectual unity. Intellectual divisions within the academy reduce its political influence--which, if it is not generic to science, is at least necessary for its own protection.

Political activity might take two distinct directions. First, the development of greater coherence and unity within a field requires that people in the field hold some influence over its sources of support. The field might, for example, establish policy committees to make recommendations to government funding agencies and look into ways of encouraging social scientists to exercise more influence in research policy matters. Secondly, some effort might be directed toward influencing legislation itself. In particular, there is need for separate legislation for the support of basic and applied research, as opposed to development and service. The Congress and the general public hold different expectations for each type of activity and persons responsible for each type will be held accountable in different ways.

As we have seen, there are several alternatives possible for restructuring and upgrading the field of educational research. But the fact is that too little is known about the institutionalization of a new research field to rely on any single strategy. A variety of research patterns must be actively supported and strengthened.

Appendix A - The Interviewees

Barber, Bernard - Bernard College, Columbia University, New York, New York.

Ben-David, Joseph - Department of Sociology, University of Chicago, Illinois.

Clark, Terry - Department of Sociology, Columbia University, New York, New York.

Cole, Stephen - Department of Sociology, Columbia University, New York, New York.

Crane, Diana - Department of Social Relations, Johns Hopkins University, Baltimore, Maryland.

Glaser, Barney - University of California, San Francisco, California.

Kaplan, Norman - George Washington University, Washington, D.C.

Kuhn, Thomas - Program in the History and Philosophy of Science, Princeton University, Princeton, New Jersey.

Lazarsfeld, Paul - Department of Sociology, Columbia University, New York, New York.

Marcson, Simon - Department of Sociology, Rutgers University, New Brunswick, New Jersey.

Menzel, Herbert - Graduate Sociology Department, New York University, New York.

Orlans, Harold - Brookings Institution, Washington, D.C.

Strauss, Anselm - School of Nursing, University of California, San Francisco, California.

Zuckerman, Harriet - Department of Sociology, Columbia University, New York, New York.

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A PRELIMINARY DESCRIPTION OF SCIENTIFIC INFORMATION EXCHANGE IN EDUCATIONAL RESEARCH

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The Johns Hopkins University

The dissemination of scientific information is an inseparable part of scientific research. During recent years considerable attention has been given to scientific information exchange and to the "information crisis" in science. Because much of this research has made it abundantly obvious that science flourishes on the personal interaction of individual researchers, information transfer in science has come to be viewed as a highly individualistic process. Information within any discipline is disseminated via many different media, and each information dissemination system serves many different consumers, each with his own needs and goals.

In this paper we wish to describe some of the characteristics of information exchange in educational research. "Educational research," as we shall discuss it, will refer to the discipline as represented by the membership of the American Edcuational Research Association.

In studying this population we have tended to concentrate on the "researcher" (as opposed to the practitioner), and our initial studies were primarily concerned with the process by which research findings are disseminated in the field of educational research. These studies have been designed to [illegible] the dissemination process from the time the producer starts the work until reports of it have appeared in secondary publications. The results of these studies are being used to construct a descriptive model of the typical dissemination system. The description, therefore, is of the information exchange activities of the average educational researcher involved in the system.**

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* In addition to the authors, the members of the staff, Kuzuo Tomita, Judith Nims and Susan Kolodny, contributed significantly to this work.

**There are sometimes large deviations from these averages and the causes of such deviations are of fundamental interest to our program. In some instances, there is evidence that there may be greater variations within a single discipline, than among different disciplines. For example, we are currently comparing the information exchange activities within a discipline of "young scientists" with those of "established" scientists, and of various kinds of researchers--e.g., administrative researchers, teacher researchers, and practitioner researchers.

In describing this general process, our primary purpose is to document some details in the hope that they might provide a basis on which to conceptualize the structure of educational research. First we shall present a description of scientific information exchange associated with one major medium, the national meeting. Next, we shall take a broader look at the dissemination process, with the scientific journal article as the critical medium. Finally, we shall present comparisons of those information exchange activities encountered in educational research with those found in other disciplines studied in our program.*

The annual meeting typically constitutes the first public announcement of a substantial portion of the work being conducted within a discipline, and the meeting presentation is a relatively early step in the dissemination process. The scientific meeting was therefore considered an ideal point of departure. Therefore, our first study of educational research focused on the 1968 Annual Meeting of the AERA. Three different groups of subjects were studied: Authors (those who made presentations at the meeting); Attendants (those who heard these presentations); and Requestors (those who requested copies of the presentations from Authors).

The information-exchange activities of Authors, in relation to the content of their presentations, are shown schematically in Figure 1. Each of the blocks in the center of the diagram represents a significant point in the Author's work. The line connecting these five points also divides the figure into two sections. To the right lie oral reports of the work. To the immediate left lie possible forms of written reports, and at the far left lie forms of secondary publications and index listings of the work.

The date of the meeting is taken as time zero; those events shown above it occurred before the meeting, those shown below occurred afterward. The ordinate on the left gives the median time (in months) of each event relative to the time of the meeting.

(1)*Work presented at the AERA meeting started about nineteen months prior to the meeting.

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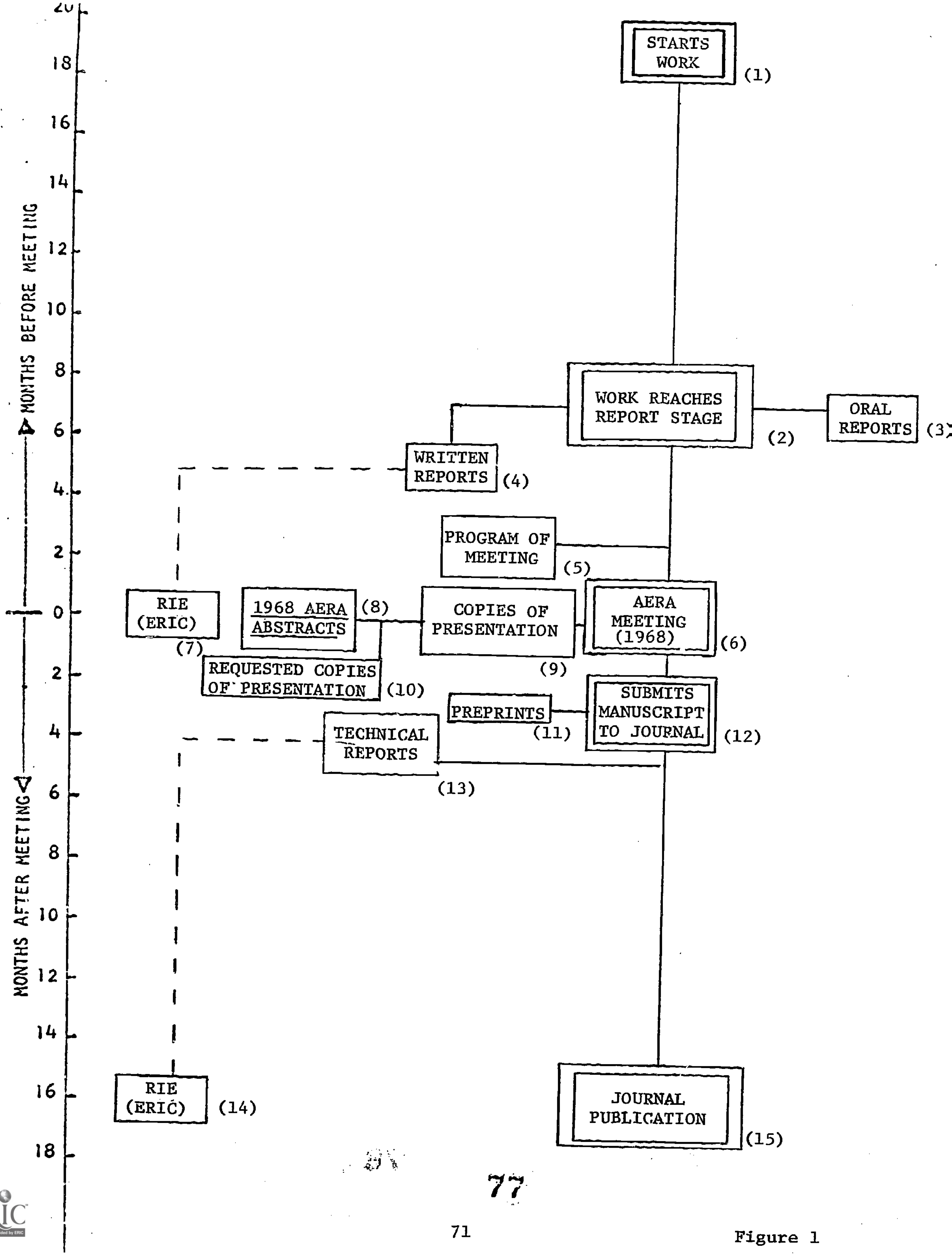
* In addition to the AERA, eight other professional societies are currently participating in the CRSC program. They are: American Sociological Association (ASA), Association of American Geographers (AAG), American Geophysical Union (AGU), American Meteorological Society (AMS), Optical Society of America (OSA), American Institute of Mining, Metallurgical and Petroleum Engineers (AIME), American Institute of Aeronautics and Astronautics (AIAA), and American Society of Heating, Refrigerating, and Air-conditioning Engineers (ASHRAE).

** The numbers in parentheses in the text correspond to events numbered in the appropriate figure.



MONTHS BEFORE MEETING
MONTHS AFTER MEETING
20
18
16
14
12
10
8
6
4
2
0
2
4
6
8
10
12
14
16
18
STARTS WORK (1)
WORK REACHES REPORT STAGE (2)
ORAL REPORTS (3)
WRITTEN REPORTS (4)
PROGRAM OF MEETING (5)
AERA MEETING (1968) (6)
RIE (ERIC) (7)
1968 AERA ABSTRACTS (8)
COPIES OF PRESENTATION (9)
REQUESTED COPIES OF PRESENTATION (10)
PREPRINTS (11)
SUBMITS MANUSCRIPT TO JOURNAL (12)
TECHNICAL REPORTS (13)
RIE (ERIC) (14)
JOURNAL PUBLICATION (15)

77

Figure 1

(2) A year later (or seven months before the meeting), the work reached a stage at which a complete report of it could be made; the first reports began to appear at this time (or shortly thereafter). Two-thirds of Authors had made some report of the main content prior to the meeting.

(3) Forty-four percent of the Authors made oral reports, usually very informal reports to small audiences.

(4) Fifty-seven percent of the Authors produced witten reports. More than one Author in five had produced a technical report and one in four, a written dissertation.

(5) The meeting program, distributed to all AERA members about a month before the meeting, offered the first "public" announcement of work to be presented. Titles and authors of presentations, but no abstracts, were included; therefore, the program was not sufficiently informative for persons who had had no prior acquaintance with the authors' work.

(6) Up to the time of the meeting, the educational research community had had little opportunity to become acquainted with the work presented.

(7) Reports of only four of the 256 presentations studied were obtainable through Research in Education, a journal of abstract reports of educational research, published monthly by the Educational Resources Information Center (ERIC).

(8) AERA Paper Abstracts, 1968, sold at the meeting and distributed by mail afterwards, may have constituted the first public report of the (abstracted) content of work presented at the meeting.

(9) Two-thirds of the Authors distributed copies of their presentations, in addition to fulfilling requests. Median number of copies distributed--60 (with a maximum of 350).

(10) During the six-week period after the meeting almost all Authors (93%) received requests for copies of their presentations (median=7; maximum=35). Only 10% of the Authors failed to fulfill all the requests received during this period.

(11) Thirteen percent of the Authors will produce technical reports with an expected distribution date of three to four months after the meeting. Such technical reports should be announced through the ERIC system in RIE.

(12) One third or more of the Authors can be expected to distribute preprints of the manuscripts they submit to journals.

(13) Three-fourths of the Authors indicated at the end of the first year after the meeting that they had either submitted a manuscript based on the main content of their presentations to journals (49.8%) or still planned to do so (28%). The median date for first submission of those manuscripts destined for publication was nine months after the meeting.

(14) Within the year after the meeting 18% of the Authors had had the main content of their meeting presentations published in journals and 13% had had their manuscripts accepted but not yet published. Based on these figures, we could not expect the majority of the material destined for journal publication to be published until two and one half years after the meeting.

This, then, is a brief description of the dissemination process from the point of view of the information producer. From initiation of work to journal publication is a long process, involving, on the average, four years. The producer goes to a considerable amount of effort to disseminate his research findings, but in most instances he fails to reach genuinely public audiences. Let us now look at this process from the point of view of the information consumer.

THE ATTENDANT GROUP

The attendant group (the first group of consumers to be discussed here) comprises a sample of persons who heard the authors present their work at the meeting. Figure 2 reflects the activities of the typical attendant at an AERA meeting presentation.

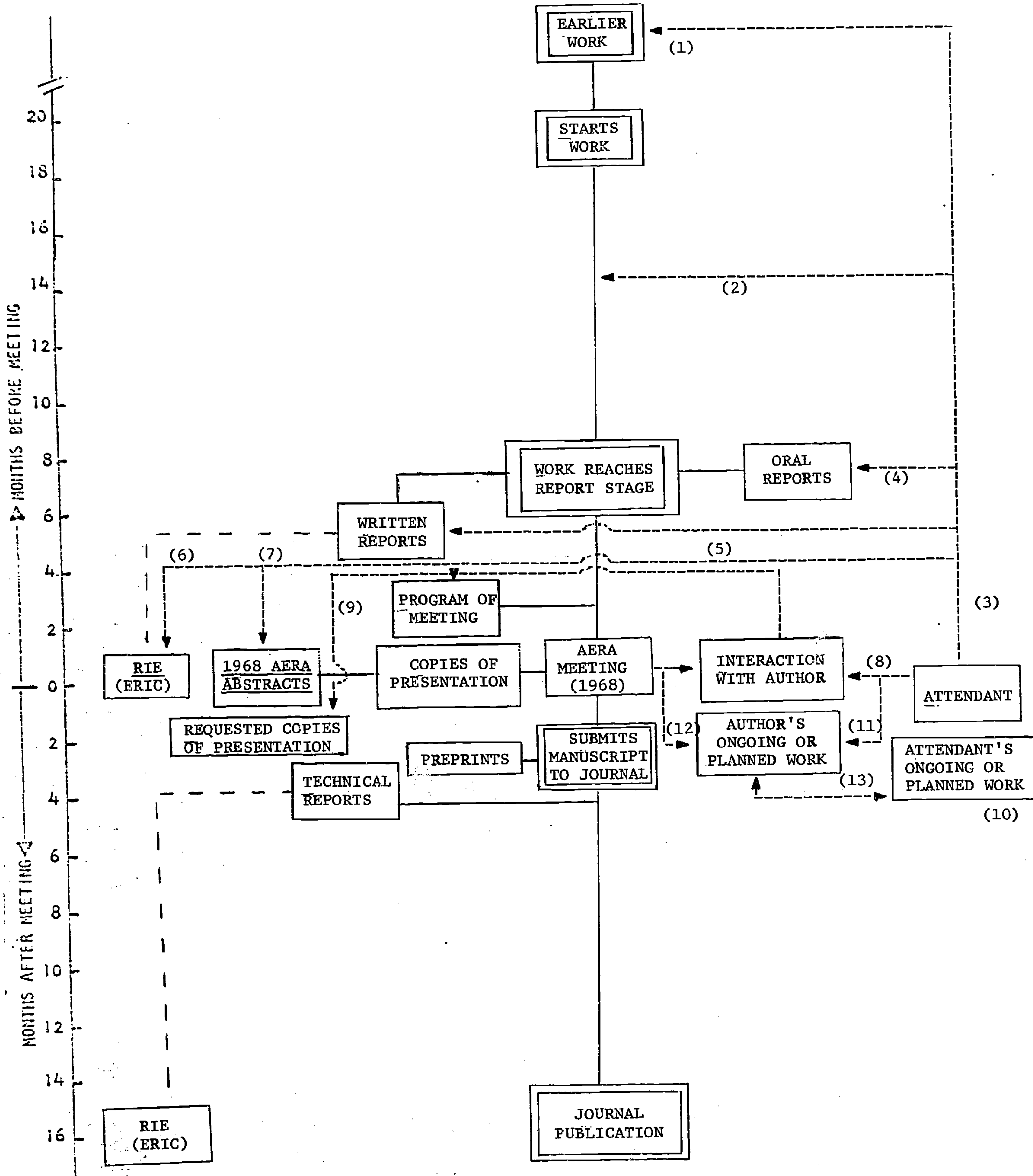
(1) Fewer than one person in ten attending a specific presentation was acquainted with any of the author's previous work (in the same area as his presentation).

(2) Fewer than one in eight was even aware, before the meeting, that the work described in the presentation was in progress.

(3) Although two-thirds of the Authors had made at least one report of the main content of their presentations before the meeting, at the time of the presentations, fewer than one Attendant in five had any prior acquaintance with the content of the presentations.

(4) and (5) Only 1.2% of the Attendants had heard an oral report of this work before the meeting, and only 1.4% had read a written report.

(6) Only 1.6% of the presentations could have been retrieved through RIE by the time of the meeting. Furthermore, only 17% of the Authors of meeting presentations were even listed, during the 13 months preceding the meeting, in the authors'



EARLIER WORK
STARTS WORK
WORK REACHES REPORT STAGE
ORAL REPORTS
WRITTEN REPORTS
PROGRAM OF MEETING
RIE (ERIC)
1968 AERA ABSTRACTS
COPIES OF PRESENTATION
AERA MEETING (1968)
INTERACTION WITH AUTHOR
ATTENDANT
REQUESTED COPIES OF PRESENTATION
PREPRINTS
SUBMITS MANUSCRIPT TO JOURNAL
AUTHOR'S ONGOING OR PLANNED WORK
ATTENDANT'S ONGOING OR PLANNED WORK
TECHNICAL REPORTS
RIE (ERIC)
JOURNAL PUBLICATION
(1)
(2)
(3)
(4)
(5)
(6)
(7)
(8)
(9)
(10)
(11)
(12)
(13)
MONTHS BEFORE MEETING
MONTHS AFTER MEETING
20
18
16
14
12
10
8
6
4
2
0
2
4
6
8
10
12
14
16

Figure 2

index of RIE.

(7) Prior to attending the meeting, only about one Attendant in nine had read the abstract of the paper in AERA Paper Abstracts, 1968.

(8) On hearing a presentation, about one Attendant in five was stimulated to contact its author about his work. Many of these contacts led to informal interaction between Attendant and author, which extended beyond the meeting.

(9) During the course of such interaction, 13% of the Attendants requested copies of the authors' presentations.

(10) About one Attendant in ten reported he was led to modify his ongoing or planned work in the same area as the author's presentation as a result of contact with the author or the content of his presentation.

(11) Interaction with authors included discussions about the authors' other ongoing or planned work, as well as, his presentation. As a result, about one Attendant in ten sought some means of exchanging information with the author on a continuing basis.

(12) These contacts had a two-way effect: over a fourth of the Authors reported that such interaction resulted in some modification of their ongoing or planned activities which were directly related to the subject matter of their presentations.

(13) Over half of the Authors reported they planned continuing interaction with persons who had contacted them with regard to their presentations; over half of these Authors had previously been unaware of the work of those Attendants with whom they planned such future interaction.

THE REQUESTOR GROUP

Requestors of meeting presentation copies were the second group of information consumers included in our study. Though less than half the Requestors in our sample actually attended the meeting, Requestors, as a group, were very much involved in meeting-related information exchange. Figure 3 shows Requestors' information-exchange activities in relation to requested copies of the presentation.

(1) Only half the Requestors had any prior acquaintance with the specific work reported in the requested papers. The other half presumably made their requests on the basis of the presentation titles and authors, as listed in the meeting program.

(2) One Requestor in six was aware, before the meeting, that

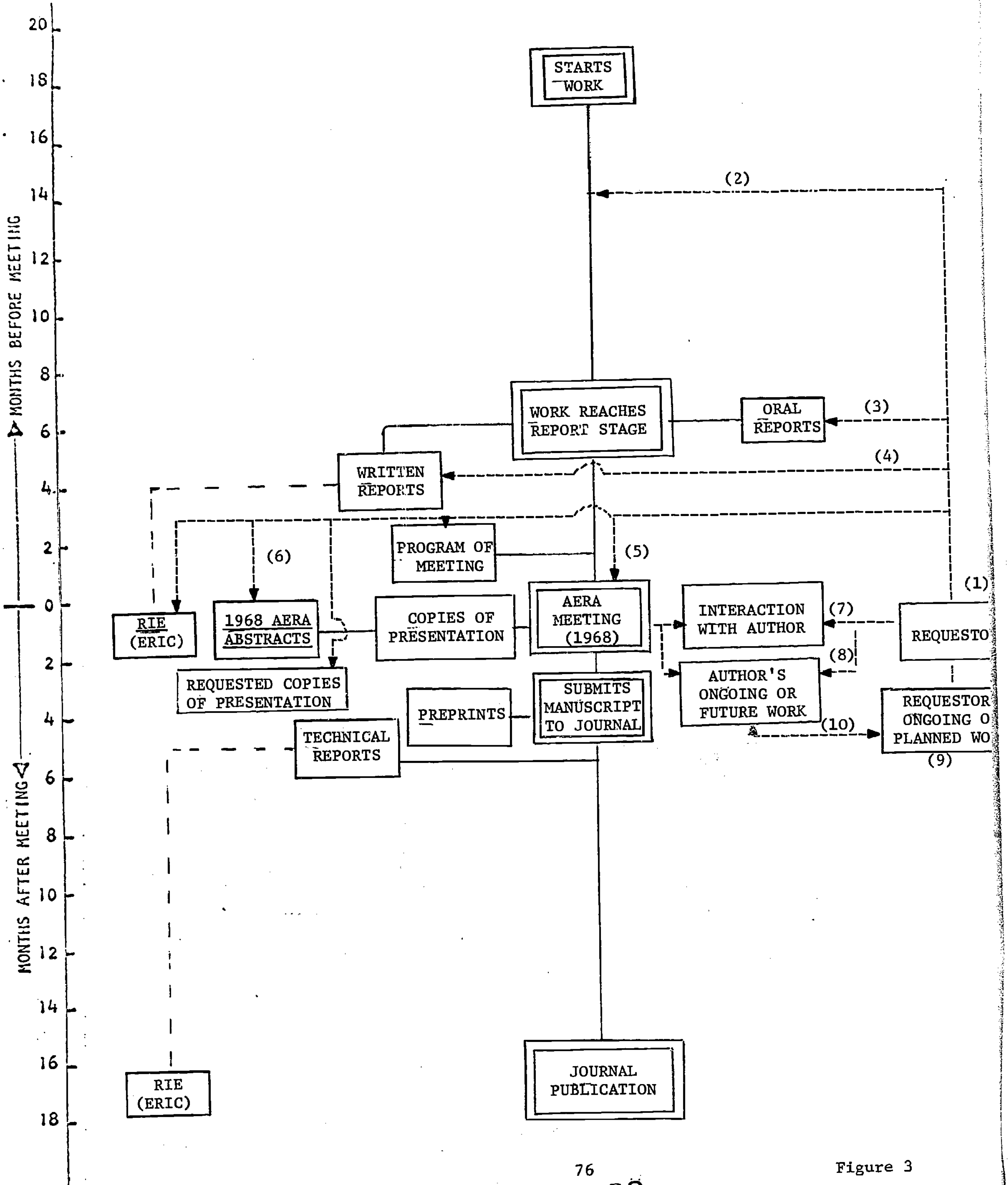

MONTHS BEFORE MEETING
MONTHS AFTER MEETING
20
18
16
14
12
10
8
6
4
2
0
2
4
6
8
10
12
14
16
18
STARTS WORK
(2)
WORK REACHES REPORT STAGE
ORAL REPORTS
(3)
WRITTEN REPORTS
(4)
PROGRAM OF MEETING
(6)
(5)
(1)
RIE (ERIC)
1968 AERA ABSTRACTS
COPIES OF PRESENTATION
AERA MEETING (1968)
INTERACTION WITH AUTHOR
(7)
REQUESTO
(8)
REQUESTED COPIES OF PRESENTATION
PREPRINTS
SUBMITS MANUSCRIPT TO JOURNAL
AUTHOR'S ONGOING OR FUTURE WORK
(10)
REQUESTOR ONGOING O PLANNED WO
(9)
TECHNICAL REPORTS
RIE (ERIC)
JOURNAL PUBLICATION

Figure 3

the author's work was in progress.

(3) and (4) Only 1% of the Requestors had heard the authors give oral reports before the meeting of the work described in the presentation; two percent had read written reports.

(5) Twenty-two percent of the Requestors had heard authors make their presentations at the meeting.

(6) Only 23% of the Requestors had read abstracts of the papers (in AERA Paper Abstracts, 1968) before requesting copies.

(7) In addition to requesting papers (generally by correspondence with authors after the meeting), 21% of the Requestors had additional interaction with authors.

(8) Such interaction largely concerned authors' ongoing or planned work in the same areas as their presentations. There was also some reciprocal information exchange: about one Requestor in six sought to acquaint the author with his own work.

(9) Twenty-four percent of the Requestors modified their ongoing or planned work (in the same subject areas as those of the requested papers) as a result of information contained in the requested papers and/or gained through interaction with authors.

(10) Ten percent of the Requestors indicated that they would seek means of exchanging information with authors on a continuing basis.

COMMUNICATION IN EDUCATIONAL RESEARCH

We have described scientific-information exchange activities associated with one medium, the annual meeting. It seems worth while to summarize some of the characteristic information-exchange activities associated with this particular meeting, before examining the general system of which it is an integral part.

First, it seems clear that few persons at the meeting had had prior acquaintance with work encountered there and that the meeting therefore, constituted the first public announcement of the vast majority of presentation material.

Second, the meeting presentation was an interim report of relatively recent work which, at the time of the meeting, was already being prepared for journal publication.

Third, the meeting exposed Attendants and Requestors to a large body of educational research of which they might otherwise have remained unaware for a year or two longer; there was, therefore, intensive information exchange with authors. This exchange primarily involved efforts to locate new sources of information and to establish new informal networks so that in the future, researchers would not have to wait to hear authors present their work at the annual meeting to learn of its existence.

Now let us examine a more general picture of scientific communication in educational research. Figure 4 focuses on journal article publication as the critical event, and the meeting presentation is viewed as one of several occasions on which authors may previously have reported the work described in their articles.

This figure is similar in format to those just presented. The times of the events, shown on the ordinate, are medians; journal publication is taken as time zero; events above journal publication occurred prior to publication, and those below occurred afterwards.

Our data resulted from a sample of 166 journal articles published between December 1967 and August 1968. These data represent only about half the amount we plan to collect for this study, but the collected data have already become sufficiently stabilized that we feel reasonably confident in reporting some of the major findings.

(1) Work reported in these journal articles was started, on the average, 34 months (or almost three years) before publication.

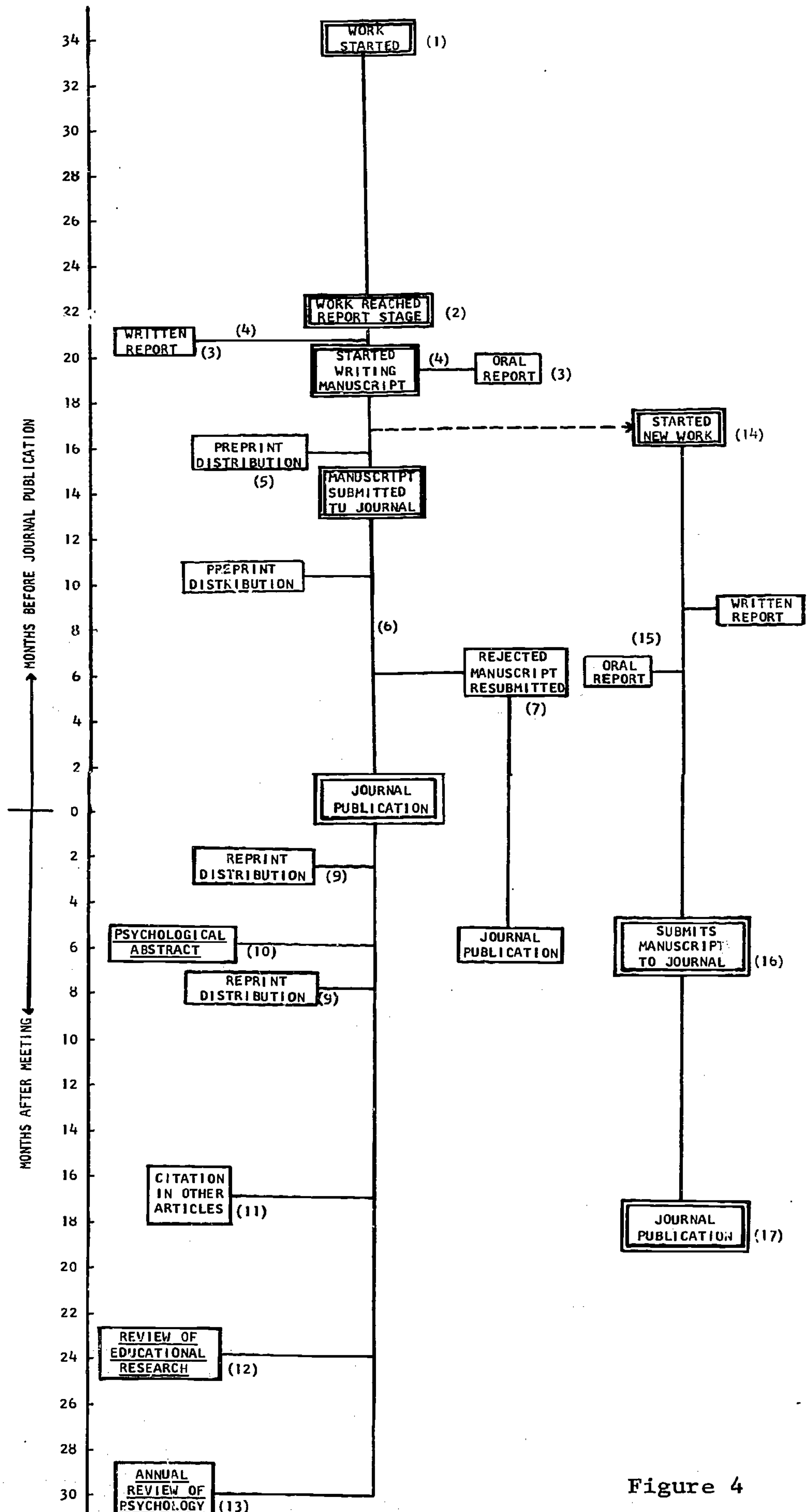
(2) Some 13 months later, or about 21 months before its publication in journals, the work reached a stage at which Authors could report the results at occasions such as colloquia within their own institutions.

(3) Shortly thereafter, they began making oral presentations and producing written reports of their new work. Figure 5 shows the types of prepublication reports the Authors made and approximately when in relation to publication of journal articles these prepublication reports occurred.

This configuration appears as might be expected. Written dissertations and theses, however, were completed, on the average, two years before their content was published in journals. In some disciplines the long delay associated with dissertations might not be cause for concern. It may be a problem for educational research, however, since 28% of the articles published in the journals studied were based on dissertations or theses.

About one Author in five produced technical reports which were distributed outside his institution. Only 10% of these had been cited in <u>RIE</u> by the time the journal articles were published.

More than one Author in six used the national meeting as an occasion for presenting orally the main content of his journal ar-



MONTHS BEFORE JOURNAL PUBLICATION
MONTHS AFTER MEETING
34
32
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
0
2
4
6
8
10
12
14
16
18
20
22
24
26
28
30
WORK STARTED (1)
WORK REACHED REPORT STAGE (2)
WRITTEN REPORT (3)
(4)
STARTED WRITING MANUSCRIPT
(4)
ORAL REPORT (3)
STARTED NEW WORK (14)
PREPRINT DISTRIBUTION (5)
MANUSCRIPT SUBMITTED TO JOURNAL
PREPRINT DISTRIBUTION
WRITTEN REPORT
(6)
(15)
REJECTED MANUSCRIPT RESUBMITTED (7)
ORAL REPORT
JOURNAL PUBLICATION
REPRINT DISTRIBUTION (9)
PSYCHOLOGICAL ABSTRACT (10)
JOURNAL PUBLICATION
SUBMITS MANUSCRIPT TO JOURNAL (16)
REPRINT DISTRIBUTION (9)
CITATION IN OTHER ARTICLES (11)
JOURNAL PUBLICATION (17)
REVIEW OF EDUCATIONAL RESEARCH (12)
ANNUAL REVIEW OF PSYCHOLOGY (13)

Figure 4

ticle. (Over a third of these Authors had made presentations at the 1967 AERA meeting.) Because most national meetings publish programs and/or proceedings, we can assume that the work described in at least one journal article in six was probably announced publicly before its publication.

As Figure 5 reveals, during the 14-month interval before journal publication, there are very few reports based on the content of the future journal article. Returning to Figure 4, one can see the probable reason for this void in the dissemination process: it is at about this time that Authors have submitted their manuscripts to journals. Also, as will be shown in a later figure, most Authors have already started new work in the same areas as their articles, and are therefore primarily concerned with information exchange related to their new work.

(4) Most Authors make prepublication reports before sending their manuscripts to journals, in order to receive feedback (and therefore to modify manuscripts) before submitting them to editors. Forty-four percent of the Authors reported receiving comments and criticisms on prior reports of the main content of their articles, and that such feed back led to manuscript modification. Oral and written reports proved equally effective--one-third of the Authors reported modifications resulting from each type. Feedback at this stage in manuscript preparation led to a larger percentage of substantive changes (e.g., redefinition of concepts) than changes in style or form; such substantive changes, however, were more frequently associated with oral reports than with written ones.

(5) About two Authors in five distributed preprints, usually before submitting their manuscripts to journals. Most of these Authors distributed preprints to colleagues working in the same area; it is interesting to note that over half of them sent preprints to persons who had in some way become acquainted with their work earlier in the dissemination process and who had requested reports of the work when it became available in written form.

Of the Authors who distributed preprints, two in five reported feedback which led to modifications in their manuscripts, usually involving changes in style or organization.

(6) On the average, thirteen months passed between submission of articles to editors and actual publication.

(7) One in five of the articles sampled had been submitted to a journal prior to the submission resulting in publication. Such rejection caused an additional delay of about six months before publication.

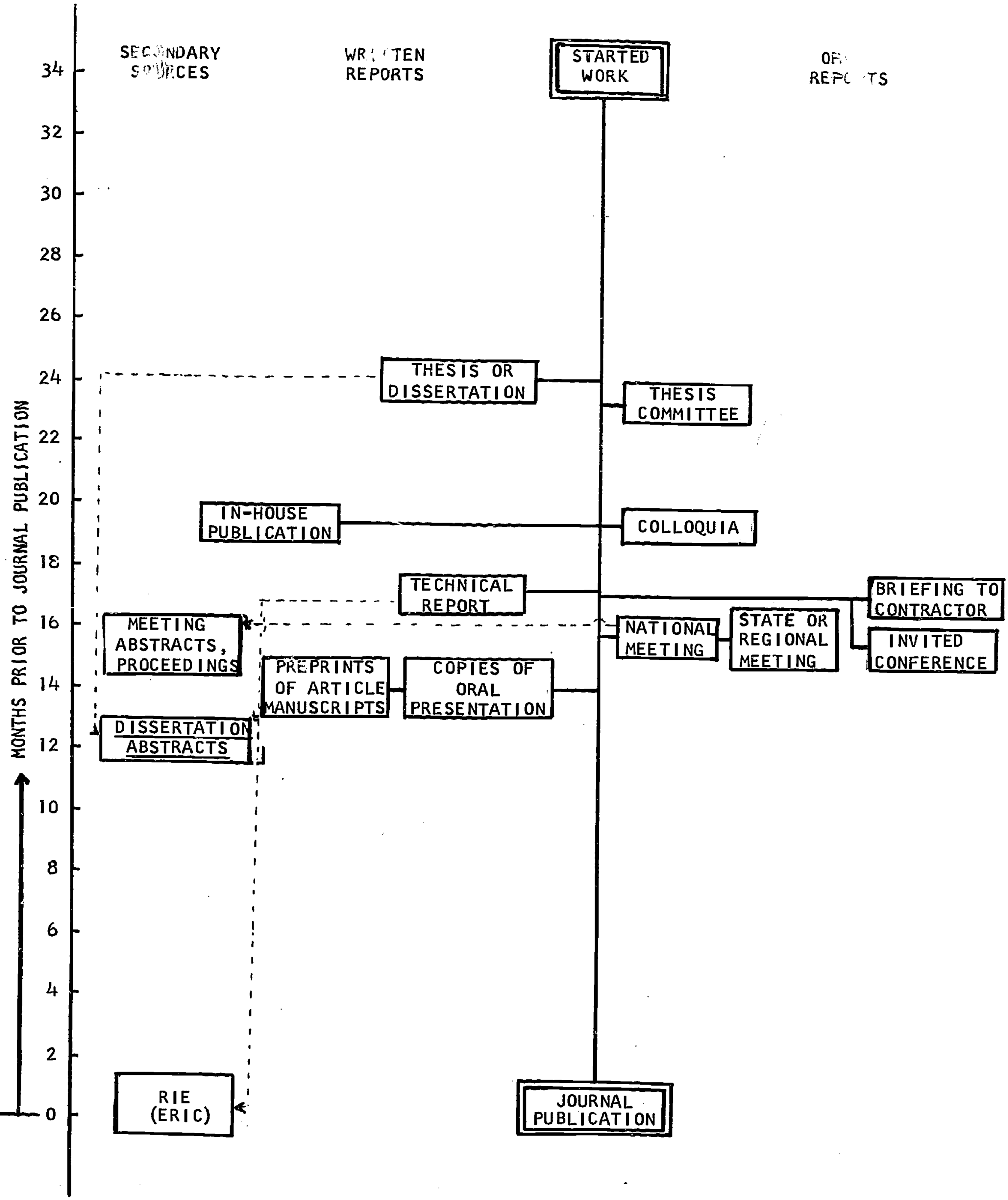

MONTHS PRIOR TO JOURNAL PUBLICATION
34
32
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
0
REPORTS
STARTED WORK
THESIS OR DISSERTATION
THESIS COMMITTEE
IN-HOUSE PUBLICATION
COLLOQUIA
TECHNICAL REPORT
BRIEFING TO CONTRACTOR
MEETING ABSTRACTS, PROCEEDINGS
NATIONAL MEETING
STATE OR REGIONAL MEETING
INVITED CONFERENCE
PREPRINTS OF ARTICLE MANUSCRIPTS
COPIES OF ORAL PRESENTATION
DISSERTATION ABSTRACTS
RIE (ERIC)
JOURNAL PUBLICATION

Figure 5

(8) Table 1 shows the submission-publication patterns of those 155 articles in our sample which were published in "core" educational research journals. From this table it would appear that two journals, American Educational Research Journal and Journal of Educational Psychology are selective in the manuscripts they publish--other core journals frequently publish manuscripts which these two reject, but these two publish relatively few previously rejected manuscripts. By comparison, Educational and Psychological Measurement and Journal of Educational Research are unselective in manuscript acceptance.

(9) The bulk of the reprints of articles ultimately to be distributed were sent out in the two to three months following publication. Most of the distribution at this time was to colleagues, via institutional or private mailing lists. However, Authors were also likely to receive numerous individual requests.

(10) Citations in secondary publications are likely to result in another wave of requests. Most of the articles included in our study will probably be cited in Psychological Abstracts four to six months after they have been published. Often an Author receives his largest number of reprint requests at this time, along with pleas for reports on any new work he may have done since.

Since the articles in our study were published in 1968, the remaining stages of the dissemination process shown in Figure 4 extend well into the future. By examining earlier articles published by the same journals, we can make the following projections about our sample.

(11) Eighteen to twenty-four months will elapse before an appreciable number of the sampled articles will be cited in other journal articles. (By the end of the third quarter of 1968, only 5% of the citations in our sample of 1968 journals referred to articles published as late as 1967, and less than 1% of the citations referred to articles published in 1968.)

(12) Some portion of these articles may be cited much later in reviews. An article selected for mention in Review of Educational Research will be cited, on the average, some two years after publication.

(13) Selection for citation in the Educational Psychology chapter of the Annual Review of Psychology involves an even longer delay (averaging some 30 months from date of journal publication), mainly because not every issue of the Annual Review contains a chapter on Educational Psychology

The dissemination process, from the time an educational researcher starts his work until that work becomes integrated into a scholarly subject-matter review, is long and arduous. The producers of scientific literature also operate as information consumers during this period. The majority of the Authors (60%) reported that by the time their articles were published they were involved in new work in the same subject-matter areas. Two-thirds of these Authors indicated that this work evolved directly from some aspect of the work described in their recently published articles.

Figure 4 shows this new work in relation to the dissemination process associated with earlier work by the same authors.

(14) Authors started their new work shortly after they had made informal reports and had started preparing manuscripts of their previous work.

(15) The new work of almost half (46%) of these Authors had already reached a report stage before their previous work was published. Half the authors whose work had reached a report stage had made a report of it some six to ten months before their previous articles were published.

(16) At the time that their previous work was being published, four out of five of those authors who had started new work indicated that they had specific plans for future publication of this new work. The majority of these Authors estimated that the new work would be submitted to a journal between six and eight months after the publication of their previous work.

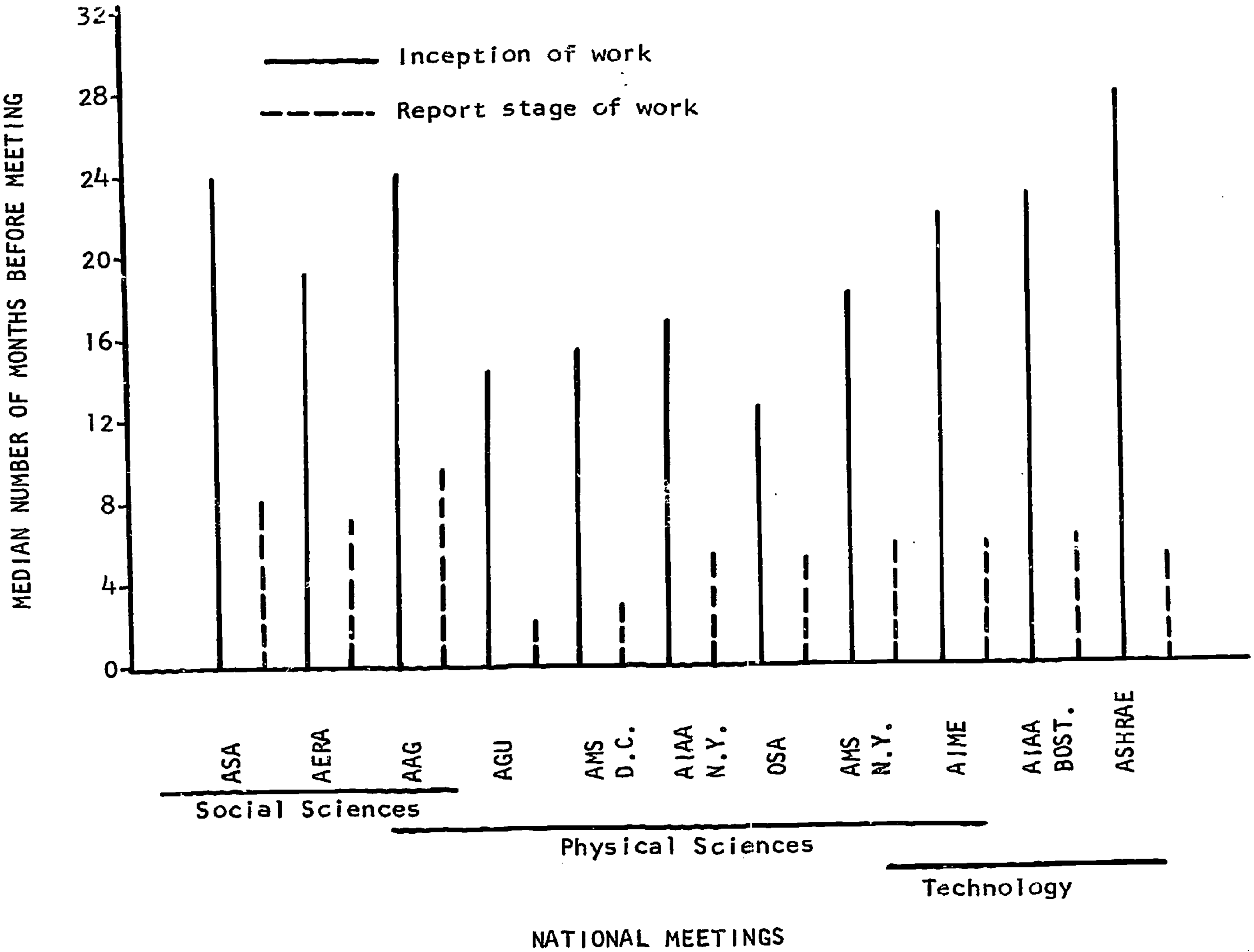
(17) Assuming that publication lags for the new manuscripts will be equivalent to those encountered for the previous ones, we can expect this new work to be published in journals some eighteen months after the publication of the previous articles.

Conclusions

We concluded, from the foregoing description, that the scientific communication system in educational research is characterized by much information-exchange activity conducted through a variety of dynamically related media. This communication system does not differ greatly from those associated with the other disciplines which we are studying; the educational research system employs similar kinds of information media and similar sequences of information transfer. Some differences, however, do exist.

In the figures following, we present findings relative to the nine disciplines we are studying. Figure 6 shows the history of work presented at the 11 meetings studies.

It should be noted that the interval between inception of work and presentation at the meeting is longer for the social sciences than for the physical sciences. The explanation is not simply that research in social science takes longer to complete. The interval between the inception and the report stage for educational research is the same as or less than this interval between the report stage and the meeting for educational research.



Inception of work
Report stage of work
MEDIAN NUMBER OF MONTHS BEFORE MEETING
32
28
24
20
16
12
8
4
0
ASA
AERA
AAG
AGU
AMS D.C.
AIAA N.Y.
OSA
AMS N.Y.
AIME
AIAA BOST.
ASHRAE
Social Sciences
Physical Sciences
Technology
NATIONAL MEETINGS

Figure 6

Figure 7 shows the percentages of authors who, before the meeting, made at least one report of the main content of their presentations. AERA meeting authors do not seem to deviate much on this measure from authors studied in connection with other meetings. We might expect that the relatively long period between the time the work was completed and the time it was presented at the AERA meeting would have offered more opportunity for educational researchers to make premeeting reports. This does not seem to be the case, however.

Figure 8 shows the percentage of presentations whose content was published as technical reports. AERA meeting presentation authors produced more than twice the precentage of technical reports than authors at other social science meetings did. Educational research would, therefore appear to have an adequate technical report literature. Results which we presented earlier suggest, however, that this literature has not been organized so as to realize its potential usefulness to the educational researcher.

Figure 9 shows AERA to be the second highest group in percentage of presentations based on theses or dissertations. While in the current view this may reflect educational research as a rapidly growing field in which most of the researchers are young. It should be remembered that most scientific disciplines share these characteristics.

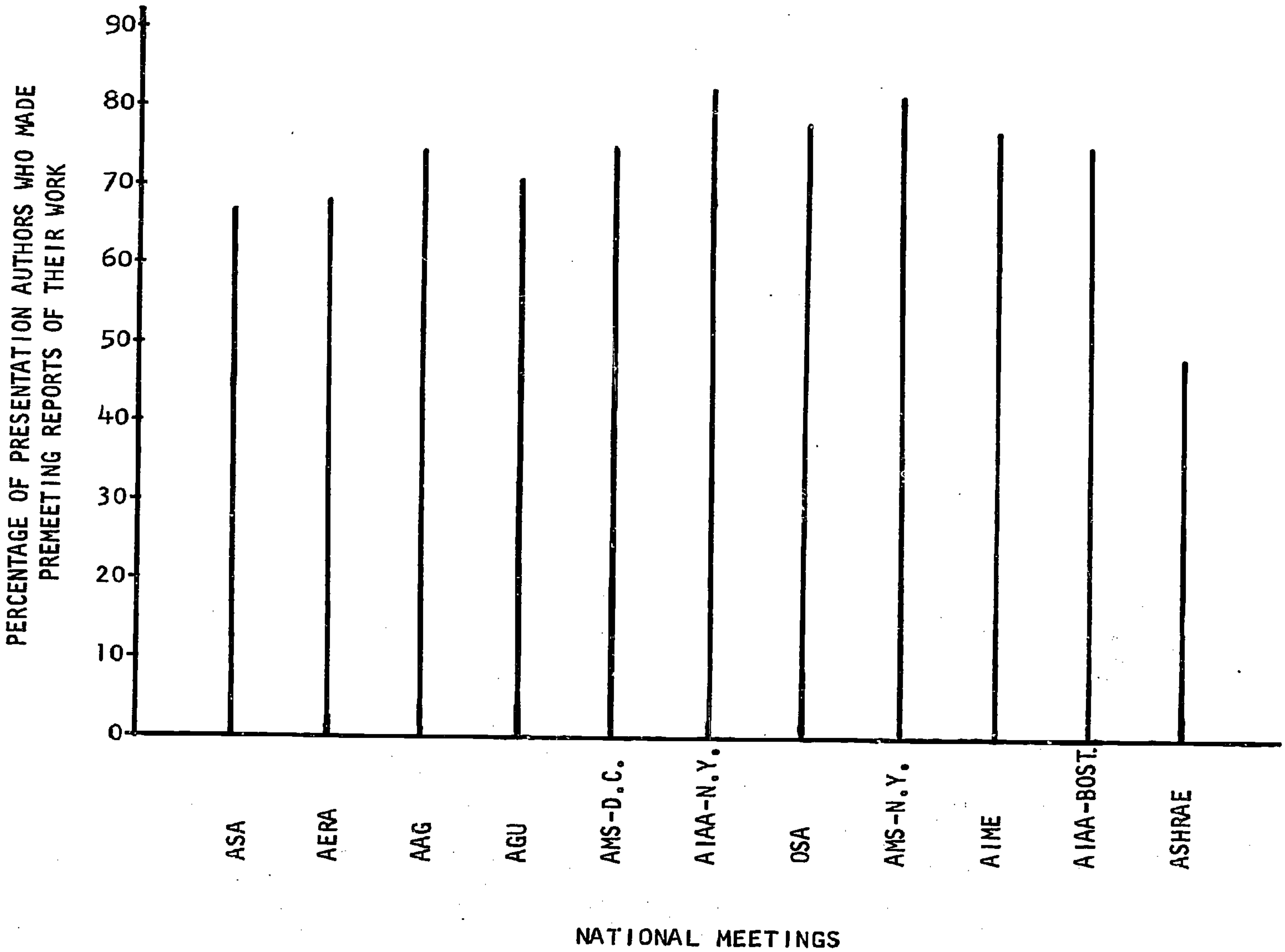
Figure 10 shows that the majority of scientists who make presentations at national meetings view these presentations as interim reports and seek publication of their work after the meeting. Educational researchers do not seem to differ greatly from other scientists in this respect.

In the preceding five figures we have presented data on the dissemination process associated with material presented at national meetings. As a whole, the process associated with educational research does not appear to differ substantially from that of other sciences. Educational researchers are clearly active in dissemination of the scientific and technical information they produce.

In the next three figures, we shall examine the dissemination process from the standpoint of the scientific information consumer.

Figure 11 shows the percentage of meeting attendants who were familiar with the previous work of presentation authors, i.e., work which the authors conducted prior to that reported in their presentations. The AERA meeting attendants are abnormally low in this respect. No doubt the lack of familiarity with authors' previous work is in part due to the large percentage (32%) of AERA presentations which were based on theses or dissertations. But other disciplines with much greater rates of familiarity also had high percentages of presentations based on theses or dissertations, e.g., AAG (39%), AIME (25%), and AGU(20%).

Figure 12 indicates the percentage of attendants who had any



PERCENTAGE OF PRESENTATION AUTHORS WHO MADE PREMEETING REPORTS OF THEIR WORK
90
80
70
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS-D.C.
AIAA-N.Y.
OSA
AMS-N.Y.
AIME
AIAA-BOST.
ASHRAE
NATIONAL MEETINGS

Figure 7

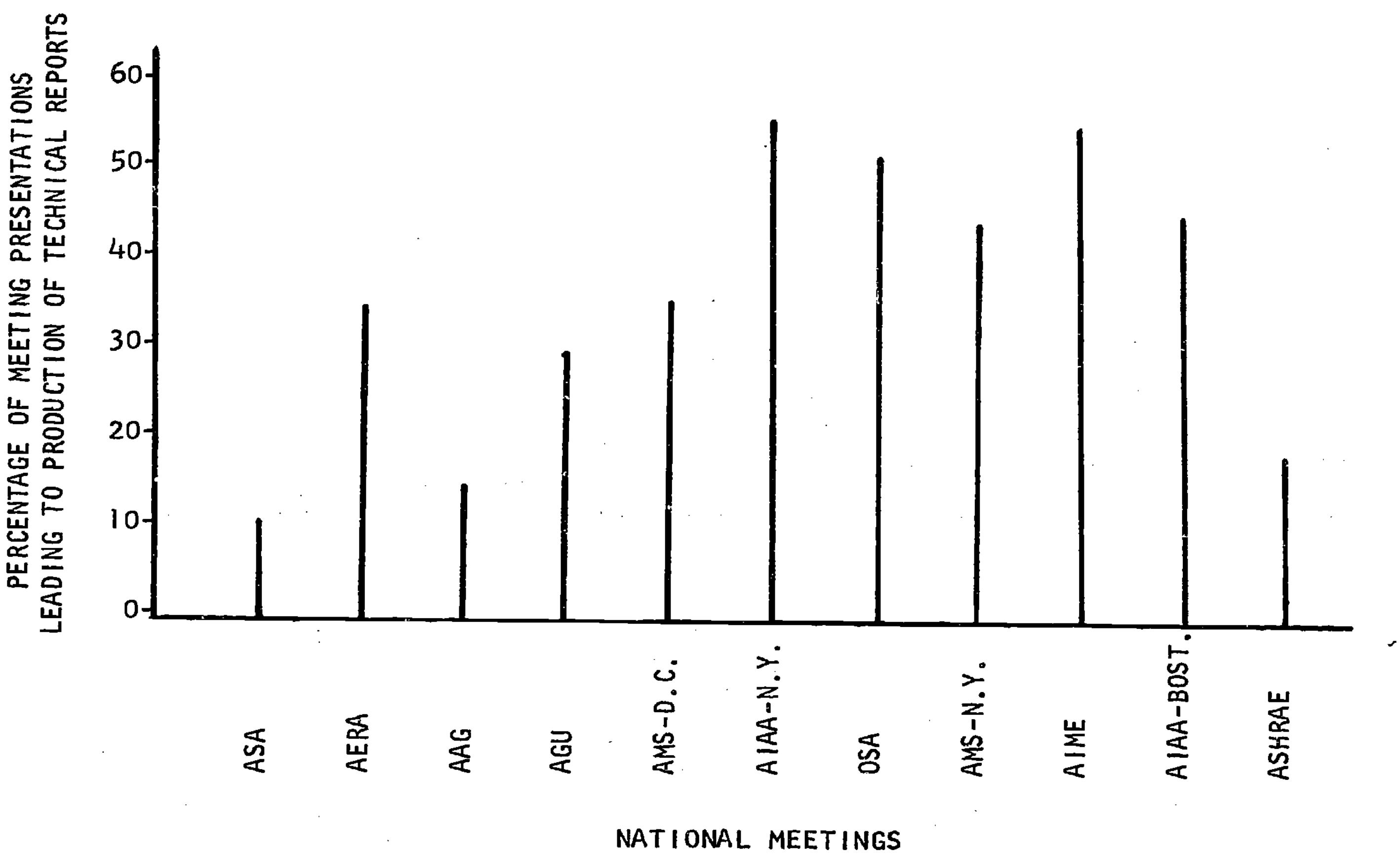

PERCENTAGE OF MEETING PRESENTATIONS LEADING TO PRODUCTION OF TECHNICAL REPORTS
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS-D.C.
AIAA-N.Y.
OSA
AMS-N.Y.
AIME
AIAA-BOST.
ASHRAE
NATIONAL MEETINGS

Figure 8

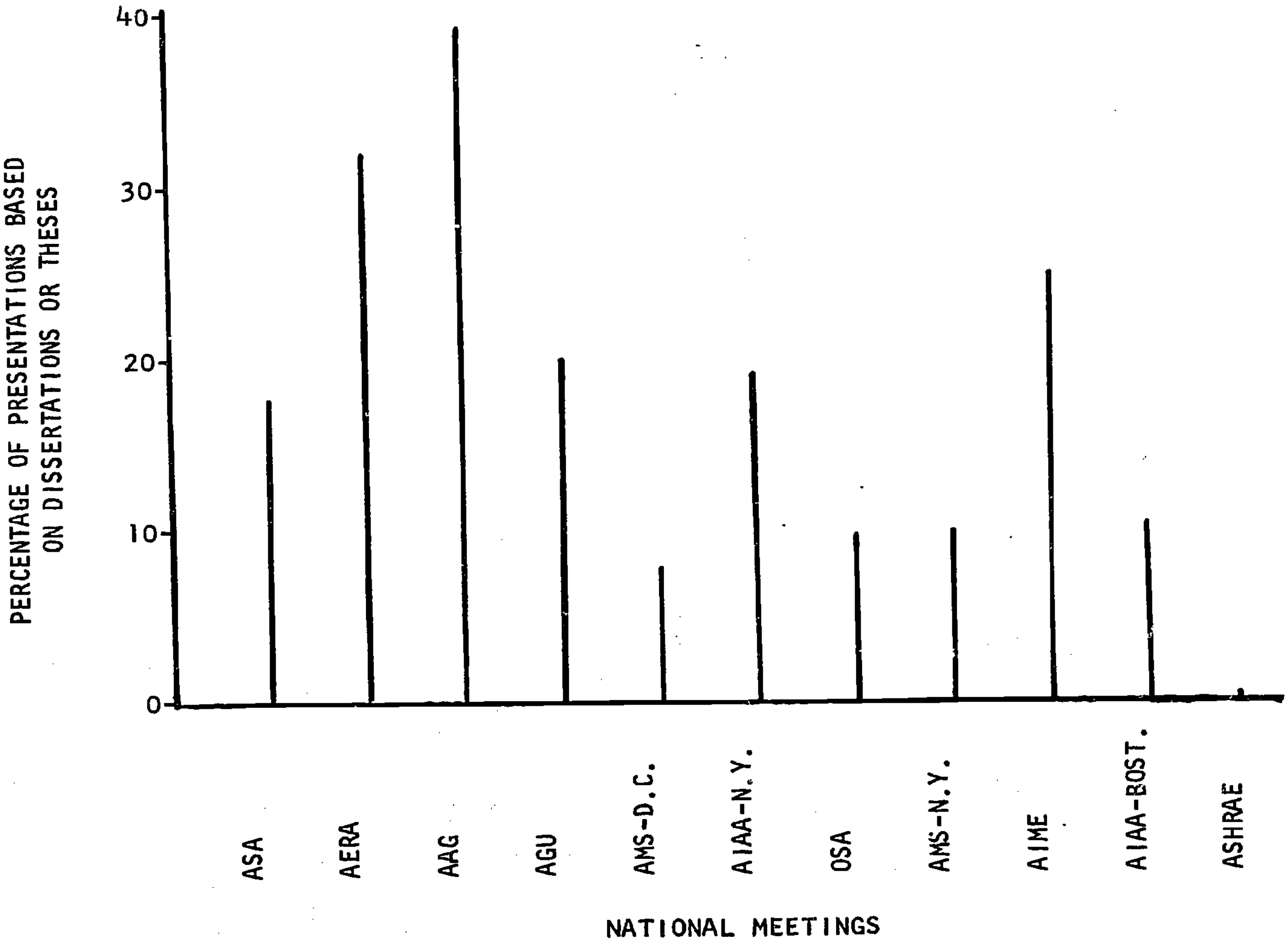

PERCENTAGE OF PRESENTATIONS BASED ON DISSERTATIONS OR THESES
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS-D.C.
AIAA-N.Y.
OSA
AMS-N.Y.
AIME
AIAA-BOST.
ASHRAE
NATIONAL MEETINGS

Figure 9

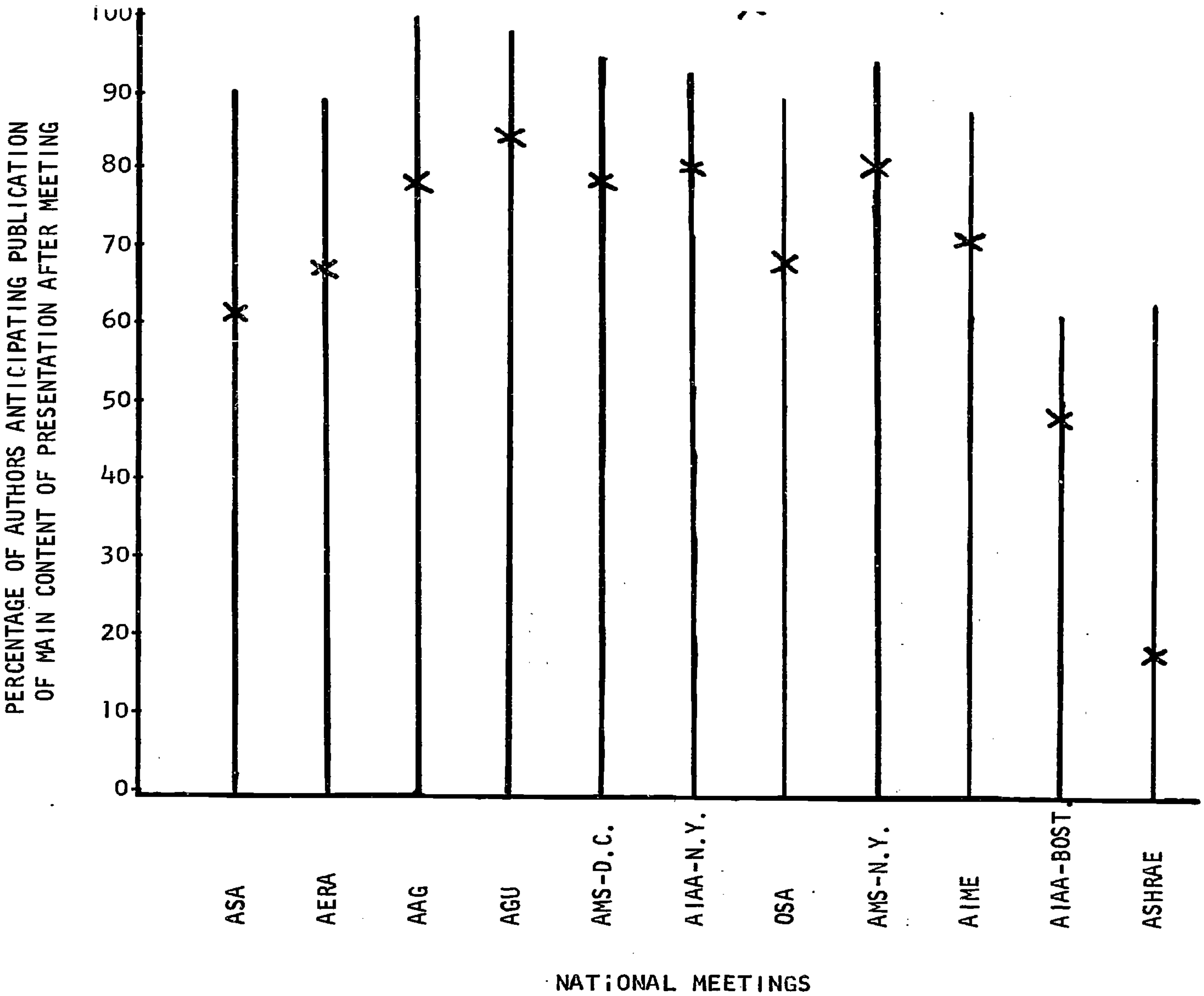

PERCENTAGE OF AUTHORS ANTICIPATING PUBLICATION OF MAIN CONTENT OF PRESENTATION AFTER MEETING
100
90
80
70
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS-D.C.
AIAA-N.Y.
OSA
AMS-N.Y.
AIME
AIAA-BOST.
ASHRAE
NATIONAL MEETINGS

Figure 10

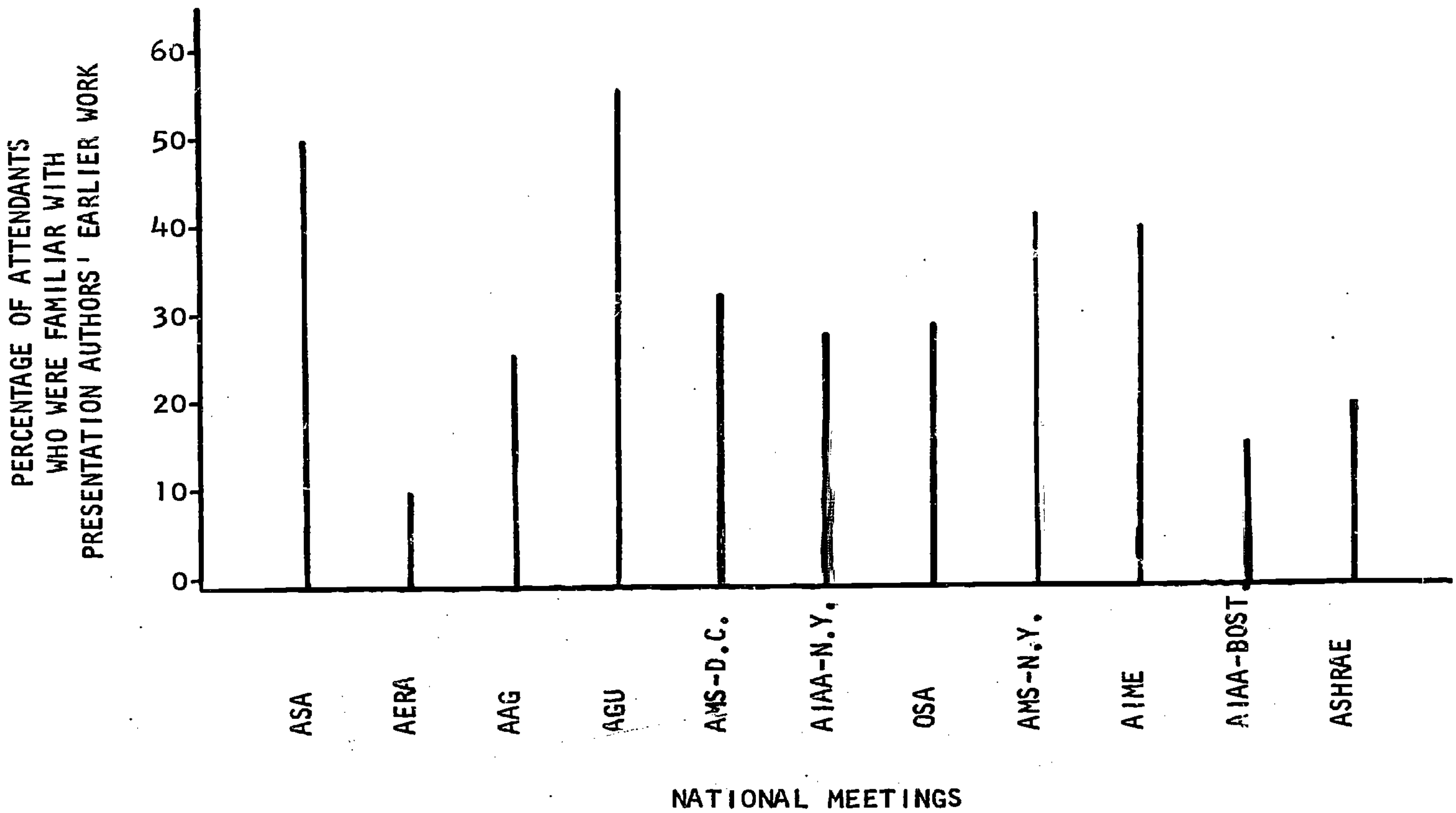

PERCENTAGE OF ATTENDANTS WHO WERE FAMILIAR WITH PRESENTATION AUTHORS' EARLIER WORK
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS-D.C.
AIAA-N.Y.
OSA
AMS-N.Y.
AIME
AIAA-BOST
ASHRAE
NATIONAL MEETINGS

Figure 11

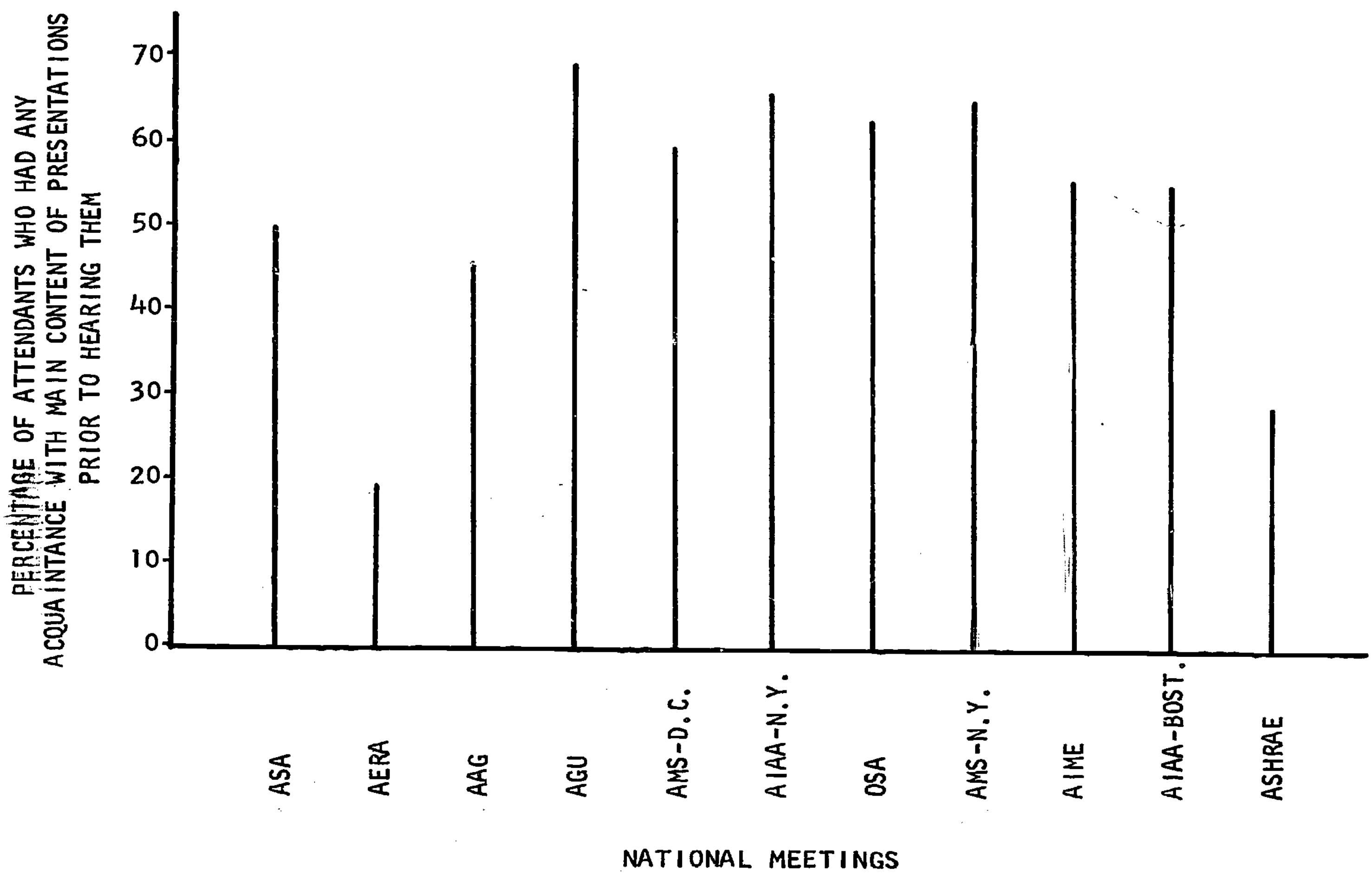

PERCENTAGE OF ATTENDANTS WHO HAD ANY ACQUAINTANCE WITH MAIN CONTENT OF PRESENTATIONS PRIOR TO HEARING THEM
70
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS-D.C.
AIAA-N.Y.
OSA
AMS-N.Y.
AIME
AIAA-BOST.
ASHRAE
NATIONAL MEETINGS

Figure 12

acquaintance with the content of presentations prior to hearing them. Here again AERA attendants were abnormally low compared with the other groups. Premeeting distribution of presentation abstracts could probably increase attendants' acquaintance with the content of those presentations they choose to attend. But even if this were accomplished, we could not expect (on the basis of data for other disciplines) that the percentage of AERA attendants having prior acquaintance with the content of presentations would be much more than double that which was found for the 1968 meeting. Apparently none of the social sciences has premeeting informal exchange processes that are as effective as those of the physcial sciences.

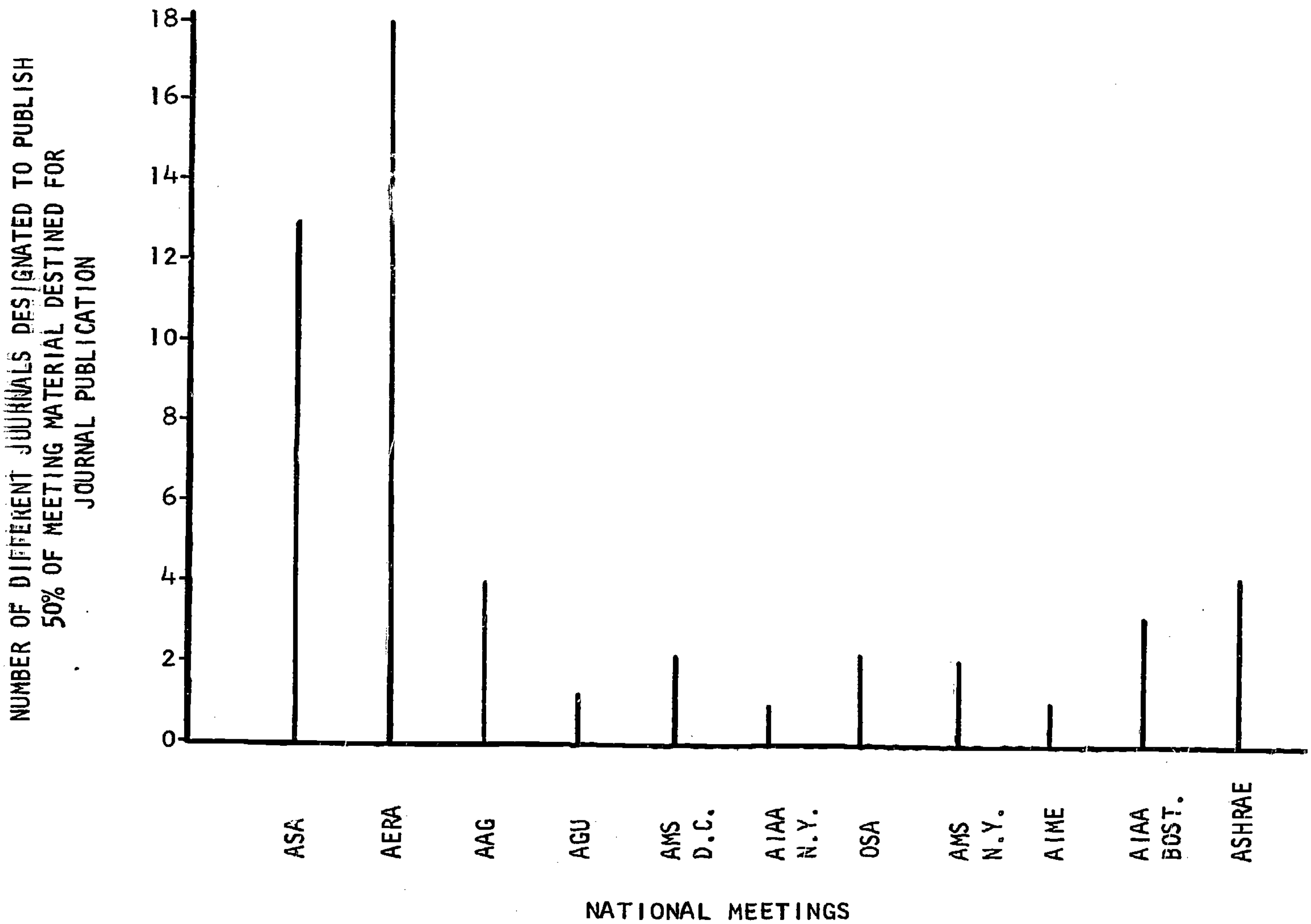
The final graph (Figure 13) in this series that is related to consumer activities, shows the number of journals designated to receive 50% of the meeting material, which was destined for journal publication. AERA presentation authors names a total of 67 different journals to which they either had submitted or planned to submit the main content of their presentations for publication. Essentially, this figure shows that an educational researcher must examine eighteen different journals in order to read half the material presented at the AERA meeting. Compared to most other groups, AERA seems extraordianrily diffuse in its range of publication vehicles. The only other discipline which comes close to educational research in this respect is sociology.

The last set of comparisons we will present is concerned with the production of journal articles. The publication of the journal article is the focal point here and the information-exchange activities presented in the figures pertain to the material published in the articles.

Figure 14, showing the percentage of article authors who had made written reports of the article content before its journal publication, shows authors of educational research material being quite active in prepublication dissemination of the main content of their articles. Two of the major types of prepublication reports are shown in Figure 15. Again educational research stands out in having a substantial percentage (29%) of its articles based on theses or dissertations; there is also a good technical report literature.

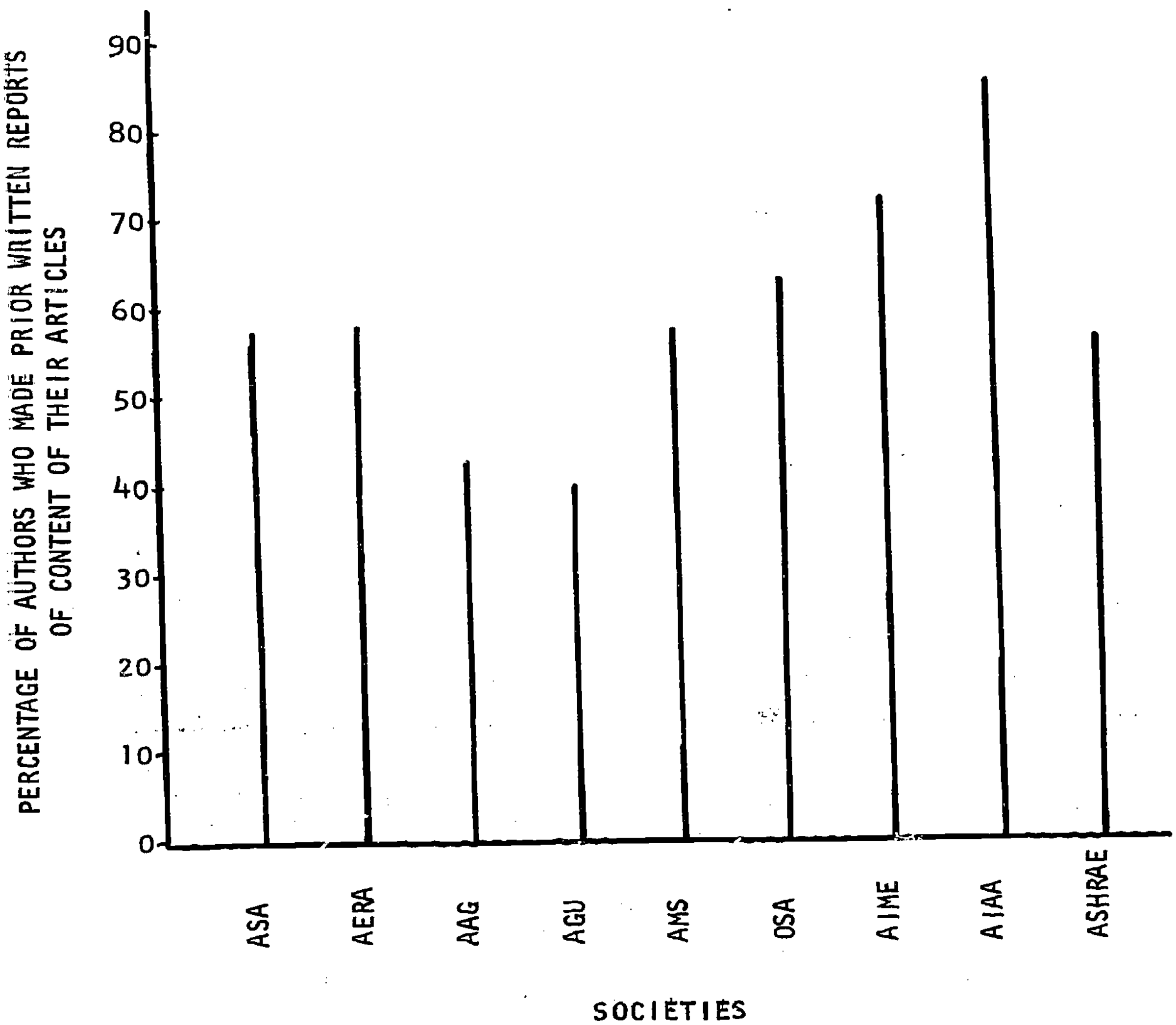
Figure 16, on the other hand, shows that educational researchers are less active than most of the other groups in making prepublication oral reports of their work. Figure 17, which shows two important occasions on which AERA authors report their work orally, suggests that the informal prepublication network associated with educational research may not be coupled effectively with subsequent dissemination media. Educational research, like the other social sciences, is extremely low in prepublication reports made at national meetings. It should be pointed out, however, that most of these groups hold more than one national meeting annually.

The social sciences are more active in making publication reports



NUMBER OF DIFFERENT JOURNALS DESIGNATED TO PUBLISH 50% OF MEETING MATERIAL DESTINED FOR JOURNAL PUBLICATION
18
16
14
12
10
8
6
4
2
0
ASA
AERA
AAG
AGU
AMS D.C.
AIAA N.Y.
OSA
AMS N.Y.
AIME
AIAA BOST.
ASHRAE
NATIONAL MEETINGS

Figure 13



PERCENTAGE OF AUTHORS WHO MADE PRIOR WRITTEN REPORTS OF CONTENT OF THEIR ARTICLES
90
80
70
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS
OSA
AIME
AIAA
ASHRAE
SOCIETIES

Figure 14

PERCENTAGE OF JOURNAL-ARTICLE AUTHORS
WHO MAKE TYPE OF WRITTEN REPORTS

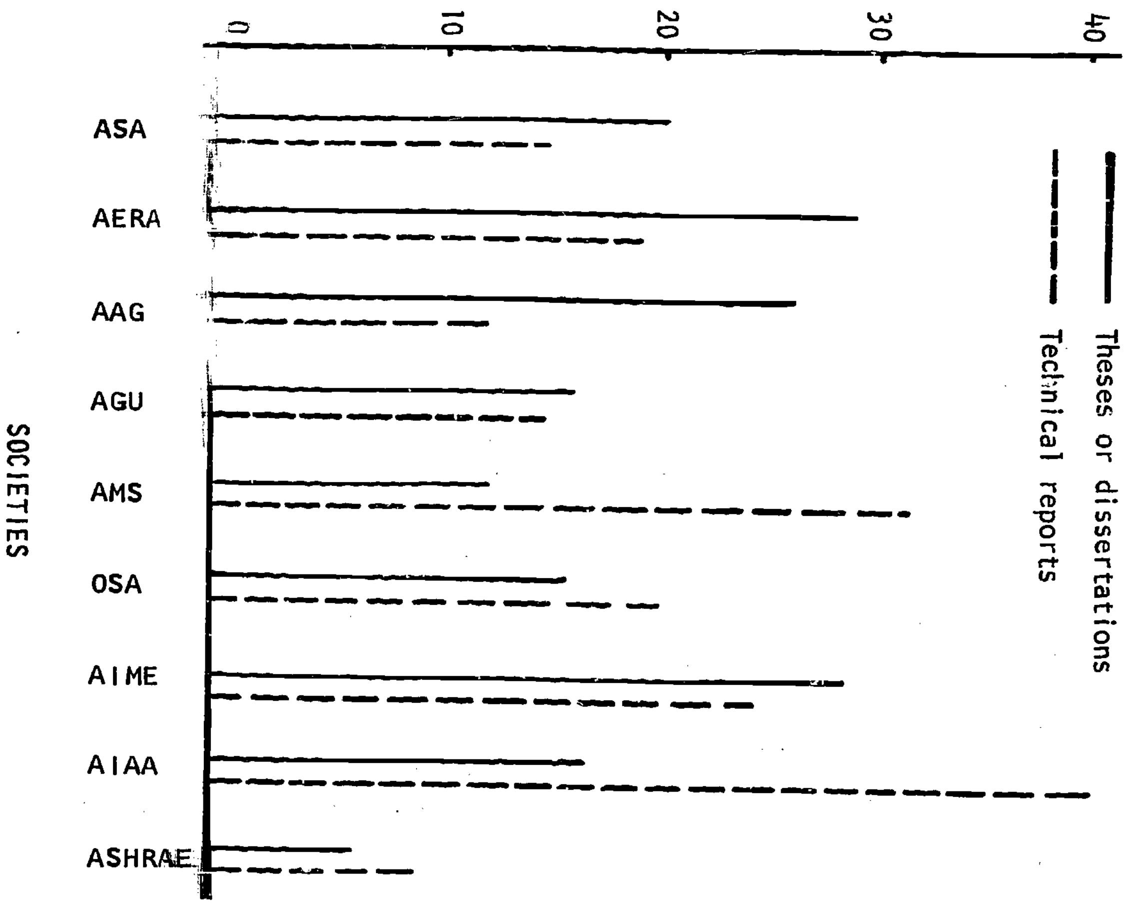


Figure 15

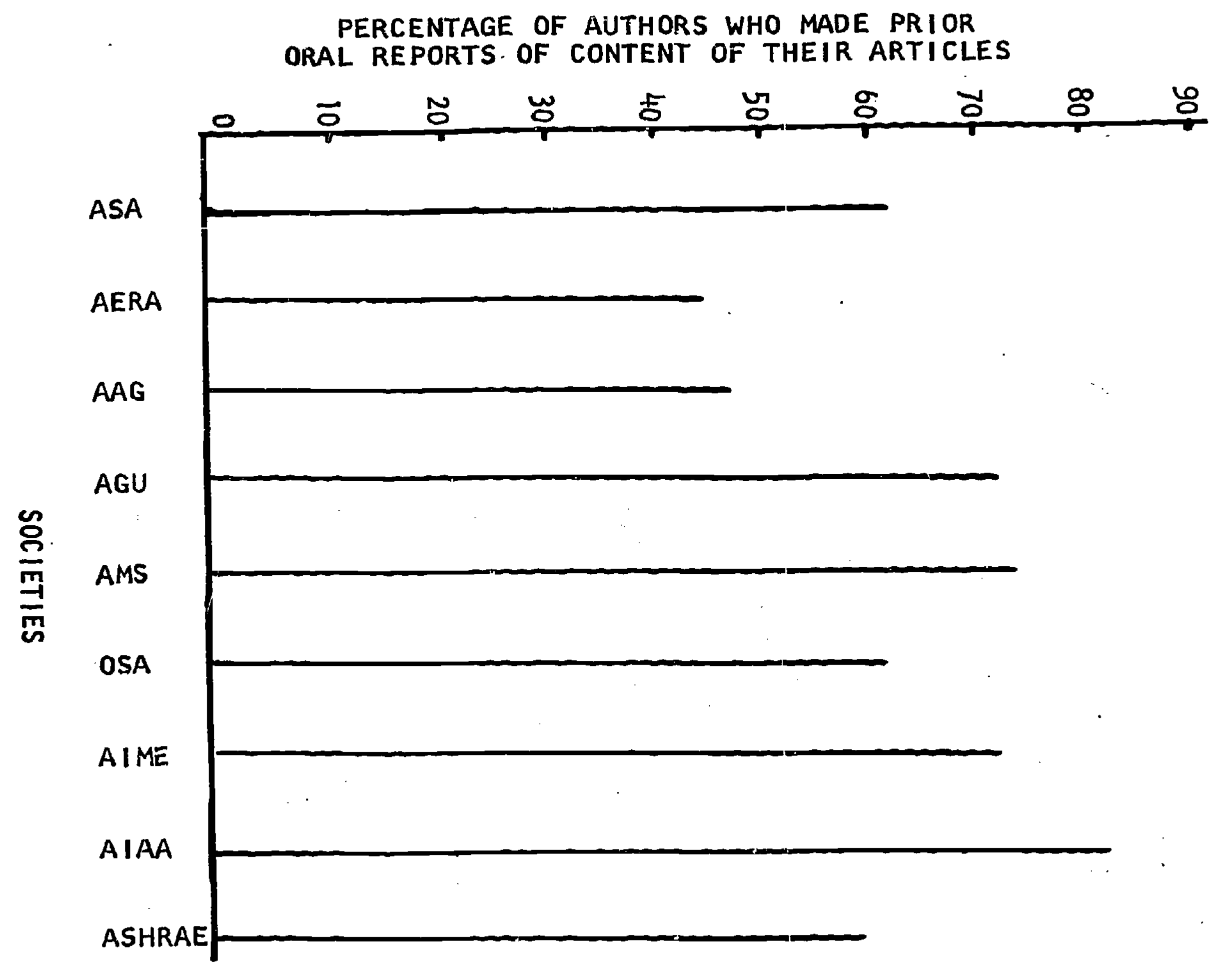
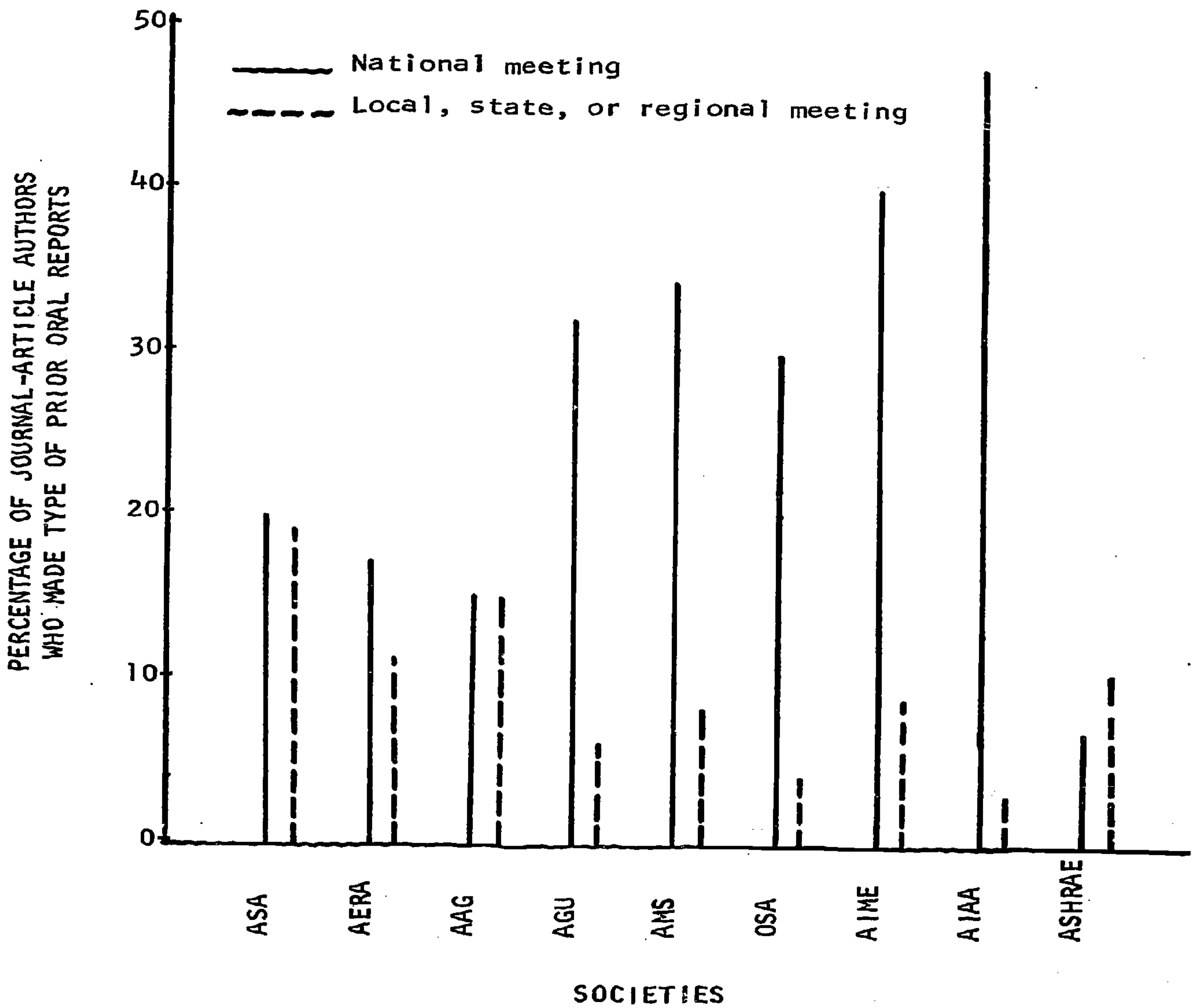

PERCENTAGE OF AUTHORS WHO MADE PRIOR ORAL REPORTS OF CONTENT OF THEIR ARTICLES
0
10
20
30
40
50
60
70
80
90
SOCIETIES
ASA
AERA
AAG
AGU
AMS
OSA
AIME
AIAA
ASHRAE

Figure 16



National meeting
Local, state, or regional meeting
PERCENTAGE OF JOURNAL-ARTICLE AUTHORS WHO MADE TYPE OF PRIOR ORAL REPORTS
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS
OSA
AIME
AIAA
ASHRAE
SOCIETIES

Figure 17

at local, state or regional meetings. While the physical and the technologically-oriented sciences also have local, state, and regional meetings at which work is reported, many of these disciplines have their national meetings tied in very tightly with their journals; so that manuscripts based on presentations at the national meeting of a society may be given special consideration by one of the journals of that society.

In Figure 18, percentage of article authors who distributed preprints shows educational researchers to be relatively low on the scale. This activity, however, is widely acclaimed as very useful by members of the physical sciences. The two main groups to whom preprints were distributed are shown in Figure 19. Educational research authors show relatively low prepublication dissemination through these channels, which suggests that educational research may not have yet developed a comprehensive informal communication network.

The lack of a fully-developed network for informal exchange does not appear, however, to mean that the informal interaction within the current network is ineffective. Figure 20 indicates the percentage of authors distributing preprints who received feedback which caused them to modify their manuscripts before submitting them to journals. Although educational researchers were less active in preprint exchange, the effectiveness of feedback resulting from what exchange did occur augurs well for the time when educational research develops a greater informal network capacity.

The final Figure 21 dramatically separates the social sciences from the physical sciences and technologies. It shows the percentage of manuscripts published in a discipline's "core" journals which had been rejected at least once prior to acceptance by the journal which published them. There are several possible reasons why non-acceptance rates are so high for the social sciences. A lack of journals is not one of them, for educational researchers appear to have an abundance of journals in which they may published the results of their research, but there is no one single large journal (with several volumes per year) covering the full spectrum of educational research as practiced by the membership of AERA.

Summary

We have attempted to present the best description available to date of scientific communication in educational research. Our assumption has been that such a description of the communication system underlying educational research would reflect the current state of this discipline. Our findings show educational research to be a newly evolving and viable discipline. The communication process associated with it may seem to embrace a good deal of random scientific information-exchange behavior when compared with the communication processes of other disciplines. The dissemination process being developed by the producers of educational research information may appear to be

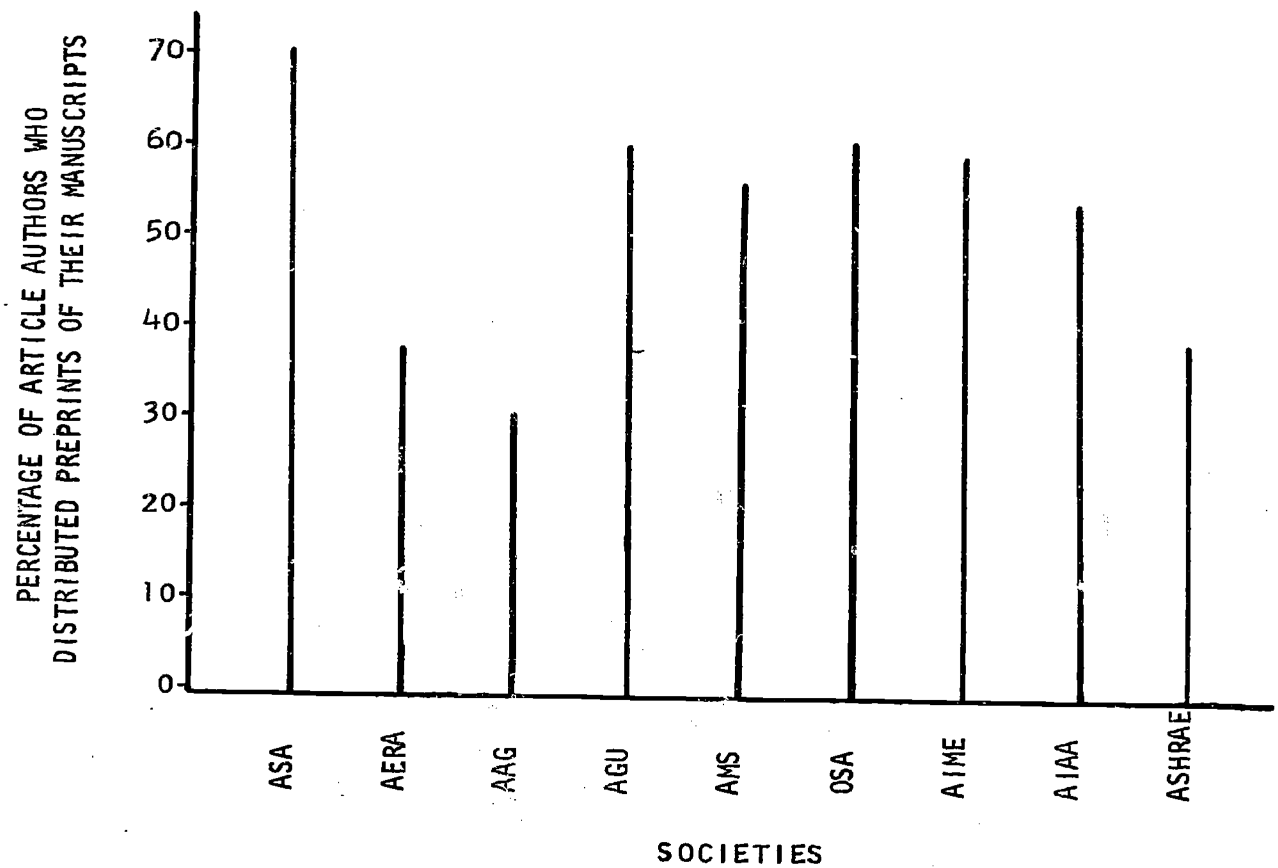

PERCENTAGE OF ARTICLE AUTHORS WHO
DISTRIBUTED PREPRINTS OF THEIR MANUSCRIPTS
70
60
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS
OSA
AIME
AIAA
ASHRAE
SOCIETIES

Figure 18

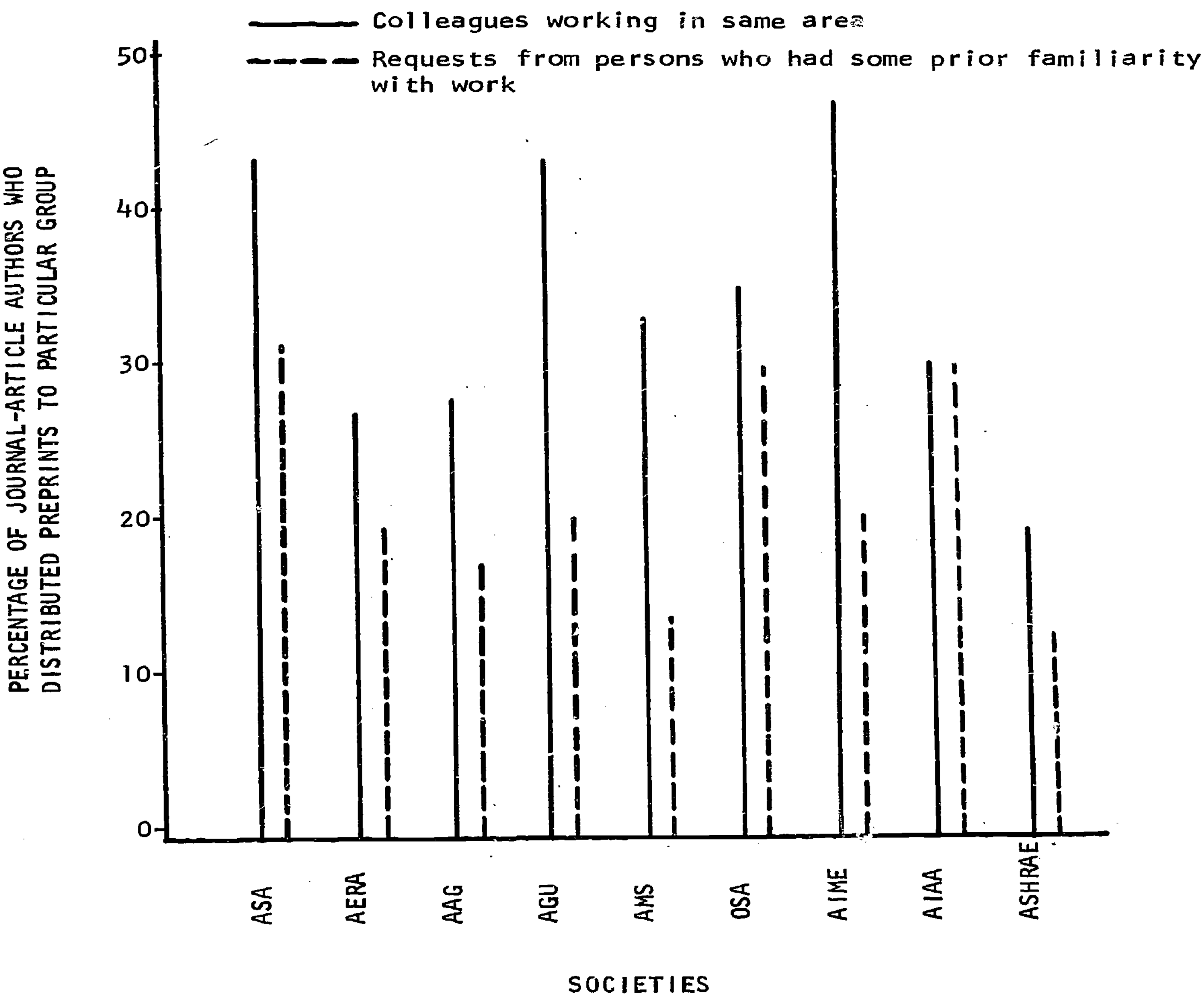
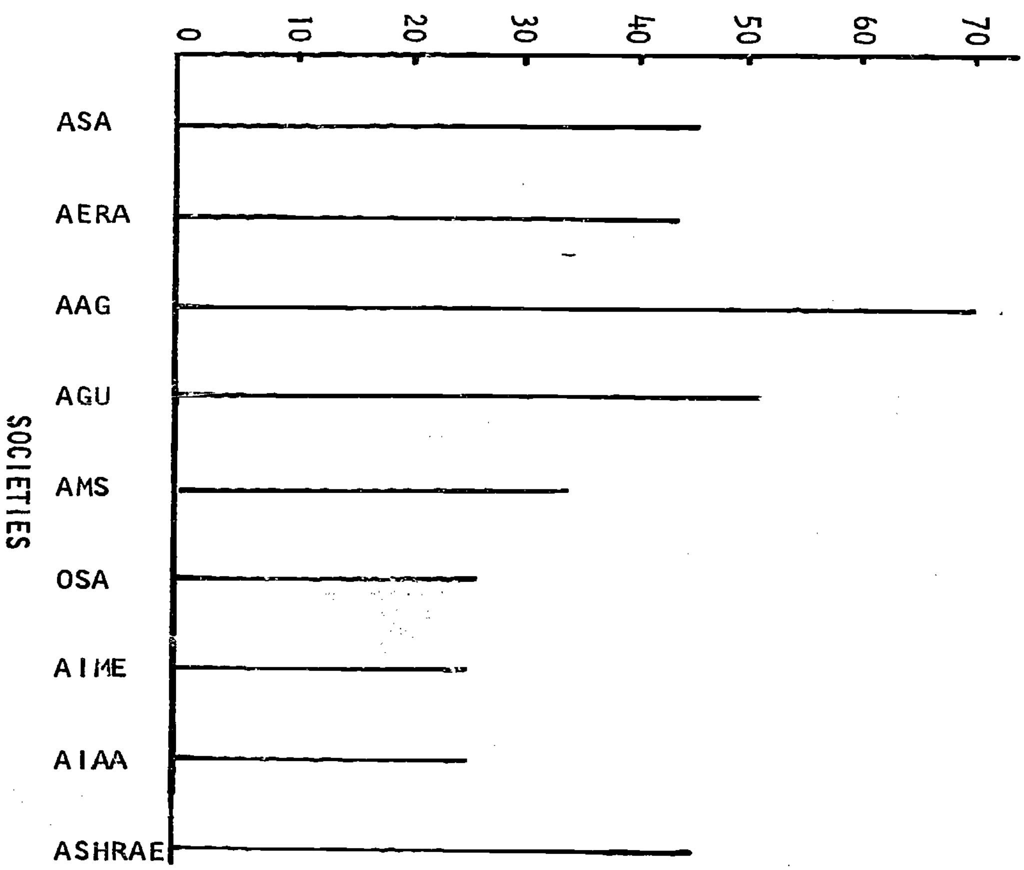

Colleagues working in same area
Requests from persons who had some prior familiarity with work
PERCENTAGE OF JOURNAL-ARTICLE AUTHORS WHO DISTRIBUTED PREPRINTS TO PARTICULAR GROUP
50
40
30
20
10
0
ASA
AERA
AAG
AGU
AMS
OSA
AIME
AIAA
ASHRAE
SOCIETIES

Figure 19

PERCENTAGE* OF AUTHORS WHO MODIFIED MANUSCRIPTS AS RESULT OF FEEDBACK FROM PREPRINT DISTRIBUTION



*Percentages are based on number of authors who

Figure 20

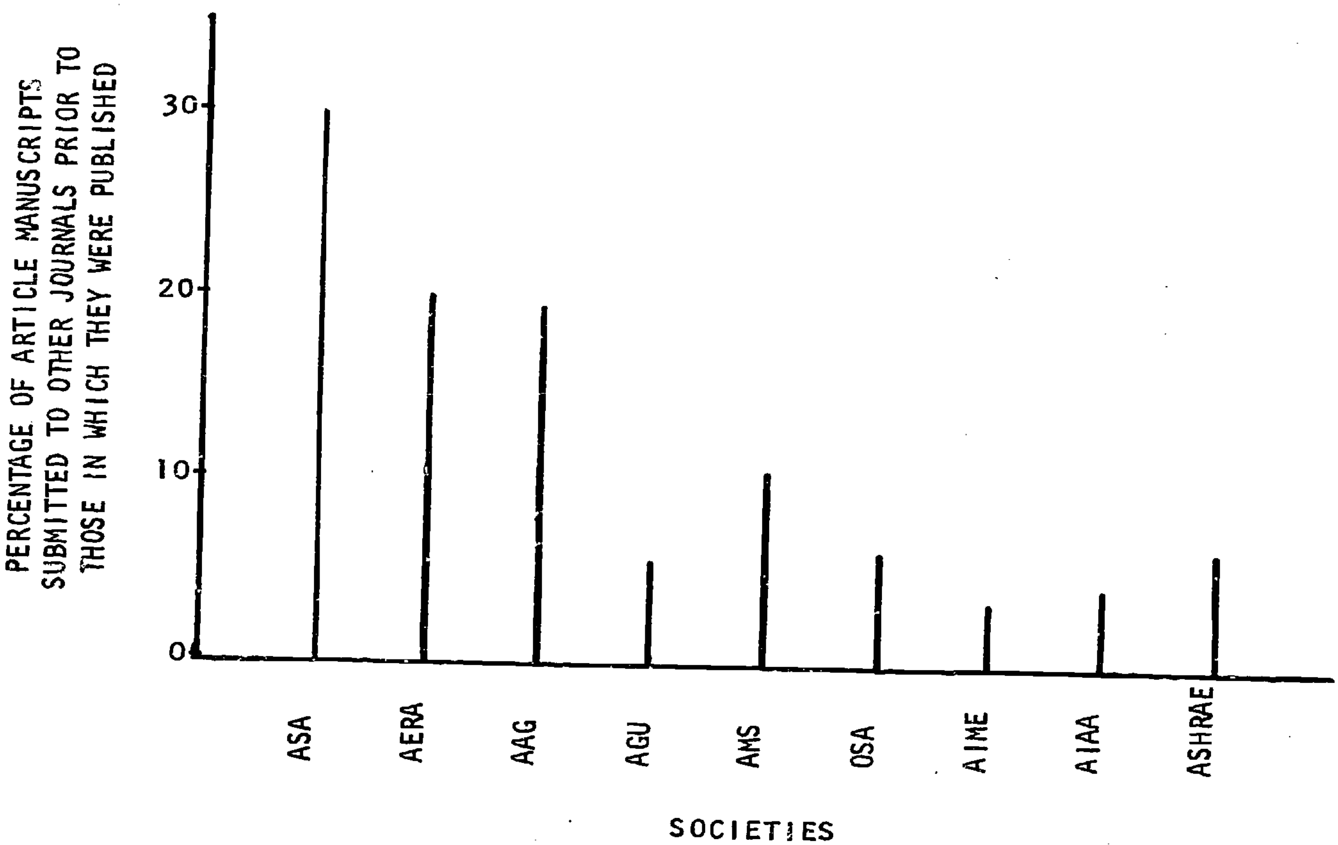

PERCENTAGE OF ARTICLE MANUSCRIPTS SUBMITTED TO OTHER JOURNALS PRIOR TO THOSE IN WHICH THEY WERE PUBLISHED
30
20
10
0
ASA
AERA
AAG
AGU
AMS
OSA
AIME
AIAA
ASHRAE
SOCIETIES

Figure 21

evolving with no consideration of the specific interest of the consumer. But it seems clear that in any scientific discipline, the producers are also the major consumers of scientific information. In a viable communication process such as educational research would appear to be developing, the system created by the information producers will somehow evolve with considerable efficiency to match the information needs of most of its active researchers.

4

THE ROLE OF INVISIBLE COLLEGES

William Paisley
Stanford University

> So we see many Wittes and Ingenuities lying scattered up and downe the World, whereof some are now laboring to doe what is already done, and pushing themselves to reinvent what is already invented, others we see quite stuck fast in difficulties, for want of few Directions, which some other man (might he be met withall) both could and would most easily give him.
>
> --William Petty, 1648

I. Big Science and the Information Crisis

In the middle third of this century, Big Science happened. From 1920 to the present, federal spending on research and development rose from 80 million to 25 billion dollars, a 325-fold increase. Science's share of the Gross National Product increased more than 30 times. Knowledge-producing manpower, including support personnel, tripled, rising from one tenth to one third of the total workforce. Following Ecclesiastes' law ("Of making many books there is no end"), the combination of funds and manpower brought in a harvest of documents that can scarcely by stored, much less organized, analyzed, and disseminated. Because the increase in information outruns solutions to the transfer problem as it has for centuries, Petty's complaint seems very contemporary to us.

One of the first signs of Big Science coming to a field is an information crisis. The information system of any field resembles a system for distributing electricity to a large number of consumers. Certain parts of the system can tolerate a hundred per cent overload, while other parts of the system fail with only a ten per cent overload. Although a convention program can be expanded one hundred per cent, if necessary, to accommodate twice as many submitted papers, no library can double its acquisition and cataloguing staff as readily (if at all). Nor can the researcher himself handle twice as many journals as before. Even if he can afford them, he can't read them.

Each field of science has discovered and discussed its information crisis in predictable succession, beginning with the physical sciences and culminating now with the behavioral sciences. Discovery and discussion do not equal solution, however, Fields with the earliest start against the information crisis, such as physics and chemistry, are only slightly ahead of latecomers, like psychology, in winning the battle.

Perhaps it is a justified oversimplification to say that the "experts" in each field divide into two factions in proposing solutions for the information crisis. One faction looks for computer assistance in making the literature encompassable again. The other faction regards formal print channels as no more than archives for students, future generations, and the most isolated researchers to consult. This faction wants to generalize its insight that the field's forward edge moves on oral and informal print exchanges directly between productive researchers.

Both factions agree on one point: adequate information systems for Big Science will evolve "naturally". In the natural ecology of information systems, policy decisions to upgrade information services are made only when the pressure on information users becomes so acute that they become uncharacteristically interested in information policy and involved in the establishment of new services. Thus the growth of research and development--following an exponential trend--is mirrored by the growth of information systems, but the latter always lags in capacity and speed. At any point in time, the distance between the two curves represents the current extent of the information crisis.

William Petty and his colleagues, the gifted amateurs who founded "The Royal Society for Promoting Practical Knowledge" 300 years ago, in 1668, faced an information crisis that was as serious to them as is ours to us. They solved it, around 1645, by forming a discussion group, a company of like-minded, irrepressibly curious students of nature. The group met informally in and around London. They exchanged ideas, reported findings of experiments, and listened to scientific news from the Continent, brought by travelers. Their knowledge and scientific leadership rivaled that of Oxford and Cambridge, and they became known as "The Invisible College."

When, after 300 years, the invisible college returns as a topic for research and for policy consideration, it seems fair to ask why. Let me, therefore, suggest conceptual models for viewing the invisible college and then offer, what I hope is, a balanced interpretation of present, and potential, roles of the invisible college in scientific information transfer.

II. Models of Information Flow

The Researcher at the Center of Cultural, Political, and Social Systems

The researcher works at the center of many systems that affect his consumption and production of knowledge. The invisible college is one such system. The role of the invisible college can be better understood if we see the array of other systems.

Systems affecting the researcher form a set of almost concentric circles (see Figure 1). The largest circle might be called the researcher within his culture. However, little control we have over it, we should not underestimate the cultural system, both as a

FIGURE 1. TEN SYSTEMS AFFECTING THE TRANSFER OF KNOWLEDGE

Cognitive system

Work team

Invisible college

Reference group

Formal organization

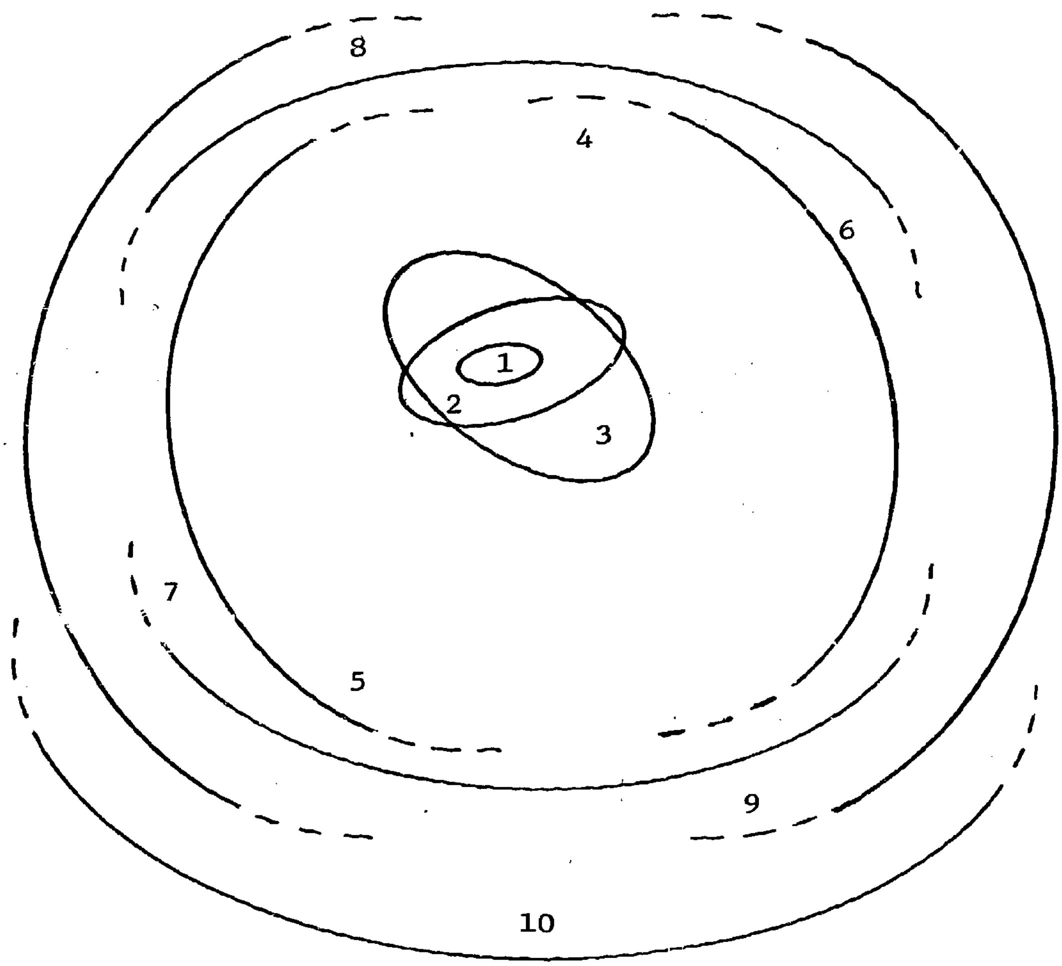
Professional association

Formal information system

Legal/economic system

Political system

Cultural system



tradition and as an ambient spirit. It is the cultural system that awards Nobel Prizes, emphasizes priority of discovery, established great private foundations, and supports universities. The effect of the cultural system is so pervasive that it tends to be overlooked. However, one can think of changes that would occur if, for example, the "priority fetish" gave way to an insistence on highly reliable knowledge. The glut of hasty reports in the system would vanish, and researchers would rely on informal channels to discuss findings that could not yet be certified for publication.

Somewhat more transitory is the researcher within a political system. Three contemporary political factors powerfully affect the American researcher. One is a scientific nationalism in many fields that causes him largely to ignore foreign research. A second is the present strength of scientific federalism: the money begins in Washington. A third is the role of the Department of Defense. More researchers in this country are hired by the military establishment than by, for example, pollution control and food research agencies. With the possibility of DOD support for projects they would like to do, researchers are drawn to available funds, frustrated by security restrictions on the flow of information, and distressed by moral issues.

Existing both within and beyond the political and the cultural systems, but affecting a smaller number of people, is the researcher within a membership group. When the researcher answers "What do you do?" by saying, "I'm a psychologist", he is locating himself within a professional membership system. Other systems may command greater loyalty, but the membership system probably controls the "official" information channels of his field. The information system of the American Psychological Association, ably studied by Garvey and Griffith (1963, 1965), is a strong example. The membership system may govern the researcher's appearance on its convention programs, may appoint him to the editorial board of its journals, and so on.

The next system is the researcher within a reference group, which includes other researchers with similar training and area of specialization, similar quality of work, and other characteristics. Whereas the researcher might not attempt to save every paper or reprint received from others in his membership group, he might well maintain a file for his reference group. Reference group identification for our researcher above might be "social psychologist studying human information processing behavior." A reference group need not be contained within a membership group; the reference group of researchers studying human information processing is drawn from several membership groups. A reference group may control a journal or two, but rarely controls an entire information system.

It should be stated here that a subsystem of the reference group system is the researcher within an invisible college. The invisible college will be discussed at length in the next section. We can continue with the researcher within a formal organization. This system emphasizes roles, lines of responsibility, and products, rather than people themselves. Both in the facilities it provides and in the policies it sets, the researcher's formal organization (that is, his employing organization) opens or blocks channels of information to him.

A subsystem of the formal organization system is the researcher within a work team. This most important information system is tuned to the researcher's problems. It documents the history of its projects in an informal and idiomatic way. Knowing what he does not need to be told, the researcher's work team provides him with rich, nonredundant information through conversation.

In this regress of systems, we come finally to the researcher within his own head. This is the system of motivation, of intelligence and creativity, of cognitive structure, of perceived relevance of information inputs and uses of information outputs. Ultimately, all other systems support his one. If nothing happens in this system, then nothing happens.

Two other, rather depersonalized systems cut across these eight. We must consider the researcher within a legal/economic system. This is a system of copyrights, patents, corporate secrecy, competitive research and development, etc.--all profoundly affecting the flow of information. In addition, the economic system determines the quality and quantity of information that other systems, such as the membership group and the formal organization, can afford to buy.

The obvious omission, thus far, has been the researcher within a formal information system--libraries, technical information centers, and the like. In most fields of science, the formal information system is actually a marketplace of competing information systems. Each finds its unique function and audience. Much like commercial air service, a network coalesces from competitive elements.

The researcher is found within many other systems, but these ten, I believe, have the greatest effect on his production and consumption of knowledge.

"Horizontal" and "Vertical" Knowledge Transfer

Only recently have we begun to distinguish between "horizontal" and "vertical" flow of scientific information and specialized knowledge in general. While we know that some information derives from "basic" research and some from "applied," that is a different dimension of difference. "Basic" research knowledge can be disseminated both horizontally and vertically, and the same is true of "applied" research knowledge.

Horizontal knowledge transfer means a sharing of knowledge at the same level of expertise. When an expert in educational statistics discusses his work with an equally expert colleague, that is horizontal transfer. When he discusses his work with a fellow researcher who is not expert in that area, the transfer is still mainly horizontal but also somewhat vertical, that is, there is exchange between two levels of expertise. When an educational researcher discusses his work with administrators or teachers, the transfer is quite vertical (See Figure 2).

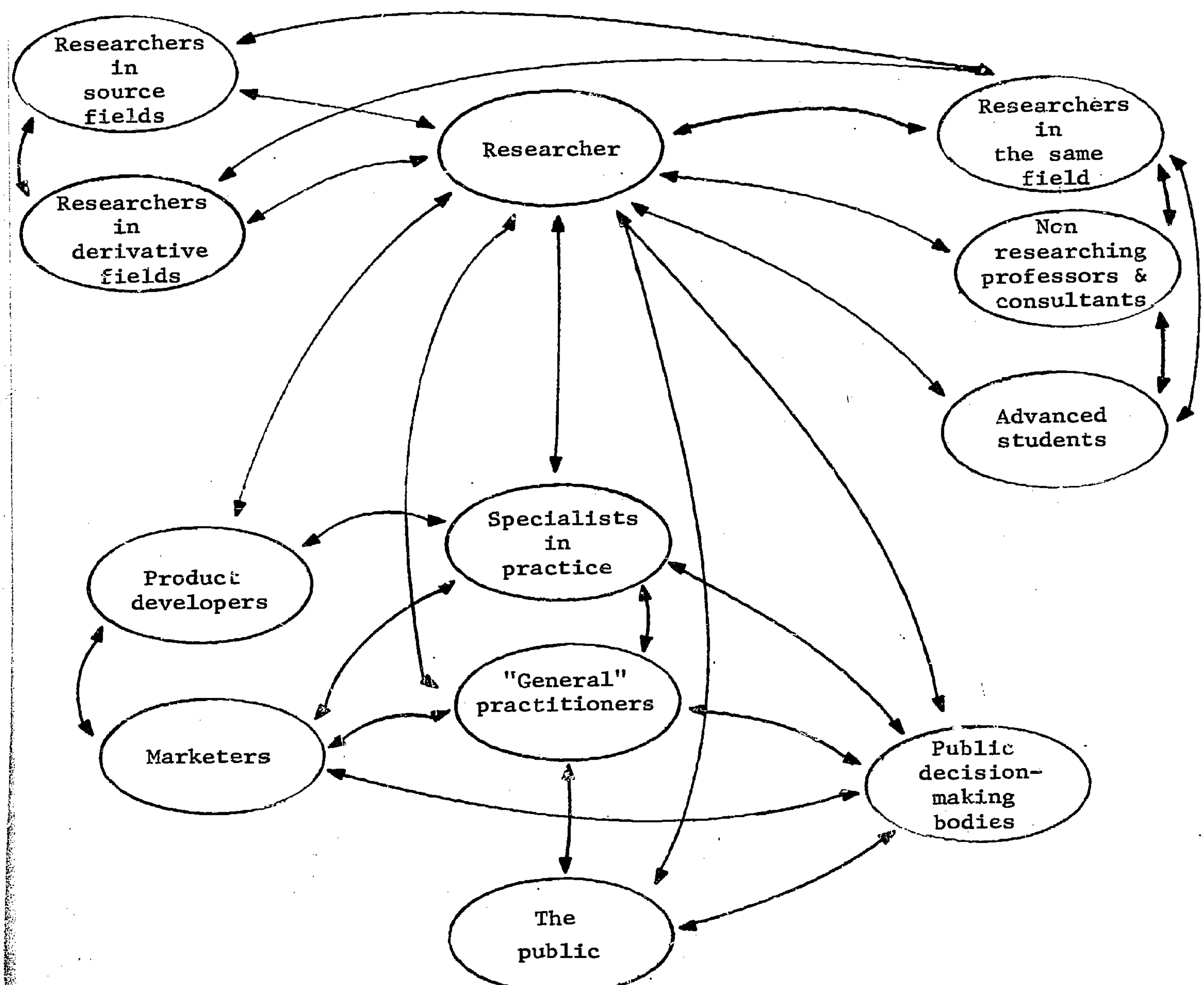
Perhaps in only one other field, public health, is the distinction between horizontal and vertical knowledge transfer as significant as in education. Public health and education are unique in their deep stratified audiences for information. Beginning with the small group of equally expert researchers, we move down one step to researchers expert in other, adjunct specialties, and to graduate students working to develop expertise in the field. Then there are non-researching professors and consultants who stay fairly close to the forward edge of the field. Below them we find administrators and practitioners of various kinds; then public decision-making bodies. At the end of the vertical line is the general public, very remote from the new knowledge that will affect it in many ways.

Consider, for contrast, vertical knowledge transfer in high energy physics (horizontal transfer is about the same in all fields). The audience for high energy physics information is shallow, perhaps three strata deep in comparison with education's six or seven strata. The physicist can talk with a colleague who is expert in his specialty. At the second level, he can talk with other physicists in general and with graduate students preparing for his specialty. At the third level are diverse audiences who are just above the threshold of comprehension--engineers who develop instruments for physicists. researchers from other fields seeking derivative or analogic value from new physics knowledge, etc.

A concern for horizontal knowledge transfer is assumed to be a mark of professionalism. We are suspicious of the researcher who subverts, or is just lazy about, the norm of full disclosure of findings. However, concern for vertical knowledge transfer hinges upon differing conceptions of professional and social responsibility. Many thoroughly professional researchers are little concerned about the dissemination of their findings downward to students, practitioners, decision makers, and the public. Invisible colleges "belong" to the horizontal knowledge transfer system and have unstudied effects on vertical knowledge transfer. At the association level, this becomes a policy issue.

By way of a footnote, we might observe that knowledge can originate anywhere on the vertical ladder. A teacher's experiences and insights, for example, are also valid knowledge in the system, fit to travel up the ladder as well as down. Having established the historical and social contexts of the invisible college, let us consider this special subsystem.

FIGURE 2 THE HORIZONTAL AND VERTICAL FLOW OF KNOWLEDGE



III. Invisible Colleges

What Are They and Why Do They Exist?

If the term "invisible college" did not already exist, it would have been necessary for Derek Price to invent it. No other term in the historian's or social scientist's vocabulary covers quite the same ground. Except in the case of some industrial research, invisible colleges extend far beyond the work team, and members of a researcher's work team may not belong to his invisible college. Membership in a reference group is almost the same as membership in an invisible college, but the essence of a reference group is shared norms, and the essence of an invisible college is shared ideas.

Price's rationale for the appearance of invisible colleges is generally well known. A very brief synopsis, from Little Science, Big Science (1963), follows:

In a normally productive field, a researcher may be able to monitor the output of a colleague group that numbers about a hundred members. Because of limitations inherent in the researcher himself, for instance, his reading speed, this number remains relatively constant even though his field doubles in size every fifteen years or so. Unless his is a very small specialty, the time comes when he can no longer monitor all of "his field". As Price puts it (p. 83):

> A noteworthy phenomenon of human engineering is that new groups of scientists emerge, groups composed of our maximal 100 colleagues. In the beginning, when no more than this number existed in a country, they could compose themselves as the Royal Society or the American Philosophical Society. At a later stage, they could split into specialist societies of this size. Now, even the smallest branches of subject matter tend to exceed such membership, and the major groups contain tens and hundreds of thousands. In a group of such size, by our previous analysis, there are likely to be a few groups of magnitude 100, each containing a set of interacting leaders. We see now such groups emerging, somewhat bashfully, as separate entities.

Price continues (pp. 84-85):

> And so these groups devise mechanisms for day-to-day communication. There is an elaborate apparatus for sending out not merely reprints of publications but preprints and preprints of work in progress and results about to be achieved...In addition to the mailing of preprints, ways and means are being found for the physical juxtaposition of members...For each group there exists a sort of commuting circuit of institutions, research centers, and

summer schools giving them an opportunity to meet piecemeal, so that over an interval of a few years everybody who is anybody has worked with everybody else in the same category.

Thus, the defining criteria of an invisible college, according to Price, are: An emergent, informal group, one hundred members or less; interacting leaders; day-to-day communication through informal media such as preprints; a commuting circuit of meetings and collaborative projects.

If this arrangement sounds clubbish and self serving,it is. The invisible college member is likely to think of other members when vacancies occur on editorial boards, proposal review panels, advisory groups, and so on. On his recommendation, organizations retaining him as a consultant will retain other college members as well. When he is "anonymously" reviewing a manuscript for publication, he will notice and approve the fashionable concepts of his groups. Crane (1967) found that authors of papers accepted by three leading behavioral science journals tended to resemble editors in academic affiliation, school of doctorate, and professional age, even when their names and affiliations were not disclosed to referees who approved the papers for publication. The jargon of an invisible college serves the group's purposes at least as well as a secret handshake. As Crane observes (1967, p. 200): "As a result of academic training, editorial readers respond to certain aspects of methodology, theoretical orientation, and mode of expression in the writings of those who have received similar training."

Although the invisible college conducts itself clubbishly, I believe that evidence can be found that membership is open to anyone who presents credentials in the form of sound training and a few exciting (but orthodox) papers. And probably no researcher whatever his credentials, can remain in an invisible college without producing. In addition to setting up opportunities for each other, invisible college members make great demands on each other's time, talent, and energy. The nonproductive member's place will be taken by an energetic newcomer--sometimes, but not often, from nowhere. In an earlier study, Crane (1965) has shown the advantages in productivity and recognition of starting at a major university under the sponsorship of a major figure in the field.

Of course "production" takes many forms, and invisible colleges prize members who give up research productivity to serve in or near seats of power, as well as members, dubbed "scientific troubadours" by Price, who specialize in carrying up-to-date news and gossip from stop to stop on the commuting circuit.

The Difficulty of Measuring an Invisible Anything

Investigators of invisible colleges are frustrated by their phantom concept, which is too reminiscent of phlogiston and ether. The historic forebear of the invisible college, chartered in 1668 as the Royal Society of London, was not that elusive. The group had regular meeting places, chiefly taverns and coffeehouses, and a succession of informal leaders. There were not enough scientists in all of Europe for the London group to lose its identity. One scarcely had to specify <u>which</u> group of scientists one was journeying to London to visit.

Today, however, it is difficult to "prove" that invisible colleges exist. Authors of invisible college studies completed thus far, wish they could find the tavern where the group congregates and raid the place. Diana Crane states the problem (1968, p.2): "Scientists have many contacts with other scientists in their own research areas and in other fields, some fleeting, some lasting. If social organization exists in a research area, it is of a highly elusive and relatively unstructured variety. It is unlikely that any particular member has a completely accurate enumeration of all members of the group."

If the operational definition of an invisible college is made very stringent, then such groups may not appear in sociometric and communication data collected within any population of researchers. The invisible college is a bad system from a measurement point of view. Its boundaries are very permeable. It makes a poor clique, because each member chooses some "outsiders" as valued friends and contacts. The fact that sociometric choices in an invisible college are not more self-contained led Mullins (1967) to conclude that a diffuse network of communication linkages continues in all directions through all fields of science. In his study of biological scientists, 254 respondents mentioned contacts in 64 different specialties. Mullins argues that the individual scientist thinks he belongs to a unique social unit because he sees only nearby linkages of an endless network.

The debate on this subject is interesting and important at the operational level of learning more about the internal and external dynamics of invisible colleges. It should not, however, be a stumbling-block. Invisible colleges raise policy questions that deserve to be considered, and I am willing to accept their existence on the basis of converging evidence now in hand. Ultimately, no one can <u>prove</u> that a certain pattern of sociometric choice and intercommunication constitutes an invisible college, but in this ultimate sense, most useful "facts" in behavioral science are unprovable.

<u>Toward a Less Simplistic View of the Invisible College</u>

Mullins, Crane, Lingwood, and others have shown us that, operationally at least, the invisible college is a complex phenomenon. To do justice to their findings, we can move toward a less simplistic view by recognizing that an invisible college can be a very heterogeneous and unstable group, sharing little more than enthusiasm for "its" research topics and a conviction that "its" philosophy of science is more nearly correct than philosophies espoused by other invisible colleges. It may be that the shared enthusiasm and philosophy of scientists stems from training at the same university or in the same tradition, but the openness of the invisible college to properly credentialed newcomers argues against the necessity of any common origin.

I shall offer some propositions about the composition and function of the invisible college:

1. The invisible college is internally heterogeneous. It contains senior members, junior members, and even student members. Sociometric analysis of the group would show a few "stars", a few "demi-stars," a few "satellites", and even an "isolate" or two–that is, someone choosing into the group, but not being chosen in return. More than one discipline can be represented, since it is present communality of interests and viewpoints, and not historical ties, that holds the group in its loose federation. The group is also functionally heterogeneous: some members are active researchers (knowledge producers) while others are serving the group administratively or politically, and still others are serving as reviewers (knowledge synthesizers) and as troubadours and scouts (knowledge providers).

2. Probably every researcher has his invisible college in a functional sense, even if certain colleges are closer to sources of funds, rewards, and power. In other words, there must be non-elite invisible colleges. The invisible college of a relatively isolated researcher may be co-extensive with his work team. Such an on-site invisible college probably resembles a geographically dispersed college in many communicative and social functions, but it would not be as rich an information sensing network as the latter. In much industrial research, the work team must serve as an invisible college. According to Marquis and Allen (1966, p. 1053). "Technologists...keep abreast of their field by close association with co-workers in their own organization. They are limited in forming invisible colleges by the imposition of organizational barriers."

3. Some invisible colleges are topic specific; others, although they may originally have pursued specific research interests, now speak for entire fields. As examples of topic specific colleges, we can name the dream research group now being studied by Susan Crawford, at the University of Chicago and the rural sociologists interested in the diffusion of innovations, a group studied by Diana Crane (1968). Examples of "general" invisible colleges can be found in leadership positions in most associations. These general invisible colleges are to topic specific invisible colleges as holding companies are to manufacturing corporations.

4. Just how many invisible colleges a researcher will belong to is

a matter of personal style and energy level. While the mode is probably one college per researcher, there are differences among fields, and perhaps even greater differences among researchers, that could raise the total in unusual cases to half a dozen or more. This is, interdisciplinary fields invite multiple memberships, just as high-energy physics does not. The most active educational researchers may have as many as two or three memberships in adjunct fields like psychology, and as many or more memberships in educational research itself. I am not speaking here of formally declared specialties, and certainly not of memberships in professional associations (an almost meaningless datum, since any researcher can amass association memberships by the dozen) but rather of imputed competence and characterization of interests by others. Researcher X doesn't belong to the psychometrics invisible college, the linguistics invisible college, and the early childhood education invisible college just on his own say-so. It is a curious fact that his membership in those invisible colleges becomes a reality when others recognize it.

5. I know of no data on this point, but the life expectancy of an invisible college is probably quite short. Those invisible colleges that have formed around an intriguing concept, like cognitive dissonance, will survive only as long as the concept continues to stimulate fresh research. When the concept begins to pan out, the college disbands and reforms in new coalitions to pursue new concepts. An invisible college focusing on a relatively unchanging topic in the field (e.g., psychometrics, reading research) may last for decades, and the senior members of the college may successfully retain their seniority, as long as fads, fashions, or authentic new developments do not undermine their knowledge and viewpoint. The longer an invisible college can survive, the greater the probability that it will receive divisional recognition in a professional association, as was the case with the recognition belatedly received in the American Psychological Association by the Skinnerians. It is probably by this avenue that some topic specific invisible colleges eventually become general and "take over" professional associations.

Data, of course, and not further speculation, is needed on these points. I predict that the invisible college will become a richer, but dirtier concept, as we identify more dimensions on which the colleges differ with differing internal consequences (stability, growth, achievement) and external effects (impact on other systems). Methodological advances are much needed if differences in invisible colleges are to appear. At the moment, a personal memoir like Watson's Double Helix tells us as much about invisible colleges as most invisible college studies do.

IV. Policy Issues

Invisible Colleges and "Horizontal" Knowledge Transfer in Educational Research

The great strengths of communication via the invisible college are relevance, timeliness, and authoritativeness. That is, the invisible college member selects information of interest to his colleagues; he passes it on while it's still fresh; and he adds his own expert opinion in an editorial comment or two.

The great drawbacks are exclusiveness; both in taking in information and in disseminating it, and insularity. That is, the invisible college is more notable for depth of coverage than for breadth--it could easily miss an important development on the periphery of its attention. Only an extraordinary college would have members posted in more than a few dozen research centers, if that many--and it disseminates primarily to itself and to privileged outsiders on the symposium and lecture circuit. Insularity has to do with excessive loyalties to one's own viewpoint and jargon, with the result that the best work done in other invisible colleges is not as well regarded as some of the average work done in one's own college.

How can we turn the strengths and weaknesses of the invisible college into an information "mission"? I am convinced that <u>each information system can carry out a certain function better than other functions and better than other systems can carry out that particular function</u>. Stated more simply; for each information system, an optimum function; for each function, an optimum information system.

This point can be illustrated with an extreme contrast. The invisible college is an impermanent system, not to be entrusted with information for long-term storage. An annual review of research, although quite permanent, is not a timely system; it summarizes research many months--even years--after projects are completed. When a researcher has a tentative finding that needs to be critically evaluated and replicated, it is more strategic to circulate the finding through an invisible college than to describe it in a formal article and to wait for mention of it to appear in an annual review. However, once the finding has been amply tested, it is strategic to secure archival permanence for it.

The unique role of the invisible college in horizontal knowledge transfer is similar to that of a consumer panel in advertising research, although the comparison is fairer if we add the collective wisdom of the advertisers themselves to that side of the balance. The invisible college reacts quickly, authoritatively, and on-target to new ideas. It identifies conceptual and methodological weaknesses in an idea before the idea is given the embarrassingly formal status of description in a journal article.

In a formal information system that is rather inelastic, the invisible college preserves some flexibility in horizontal communication by facilitation contacts between those who know and those who need to know, as well as by treating ideas interactively rather than on the once-only submission basis of the formal systems. No other system can move as quickly to take up the slack when an existing information channel fails or when new, previously unchanneled information appears and demands assimilation into the field. A system like a professional association can identify the information gap and form a policy to deal with it, but the inertia of an association is great in comparison with the light-footedness of an invisible college.

The contribution of the invisible college to horizontal knowledge transfer will be maximized, then, when the particular information function served by the college exploits its sense of relevance, timeliness, and authoritativeness, when its weaknesses of exclusiveness and insularity are not crucial, and when its ability to react quickly and to act as a switching mechanism among researchers is used to advantage.

Invisible Colleges and "Vertical" Knowledge Transfer in Education

Vertical knowledge transfer is usually a massive undertaking, making use of print and electronic channels whenever possible and involving personal contacts only at the destination, where "change agents" and other kinds of opinion leaders help to overcome "information apathy" by taking information agressively to the ultimate receiver. If invisible colleges have a contribution to make in vertical knowledge transfer, the relevant attribute can only be authoritativeness or perhaps prestige. All the attributes of invisible colleges that make them effective in horizontal transfer are irrelevant in vertical transfer, with the exception of the attention-getting attribute of prestige and the persuasive attribute of authoritativeness. That is, the ability to speak for a field that brings a general invisible college into leadership positions in an association also commands some attention in audiences of outsiders. But except in rare instances in which the effect of the whole is greater than the effect of the sum of the parts (as was probably true in the invisible college of nuclear physicists working in the Manhattan Project during World War II), we are really discussing an individual attribute and not an attribute of any invisible college. Only rarely does a group of researchers come across to the public as an invisible college. Probably the world wide group of surgeons performing heart transplants is the best current example. The rest of the time, a researcher speaks only as an individual, however prestigious the invisible college to which he belongs.

Invisible Colleges and a Political System like the U.S. Government

The theme of an optimum match between information systems and information functions provides clear guidelines, I think for dividing the information burden between the invisible college system and the political system. In the age of Big Science, only the federal government can organize and finance an archiving operation that preserves nearly all the print literature being generated in each field. We have seen a steady evolution of responsibility for this operation, from individual researcher to individual libraries, to professional associations, to consortia of associations (such as the Federation of American Societies for Experimental Biology), and finally to the federal government. As the archiving problem has grown, it has been bucked up to higher and higher levels of centralization and available resources.

The philosophy of the Committee on Scientific and Technical Information (COSATI), organized under the office of the President's Science Advisor, has been influential in this regard. COSATI argues that each federal agency that supports research has some role to play in managing and disseminating the resulting information. Certain agencies are designated as "responsible agents" for all domestic information in a field of research. Examples are the National Library of Medicine, for biomedical research; the National Science Foundation, for chemical research; and the Office of Education, for educational research.

With or without a COSATI position in this matter, it seems inevitable that the archiving operation become the responsibility of the federal government or that it be abandoned altogether. In many instances the government will be taking over an archiving operation previously handled by professional associations. (Occasionally it will make sense to both parties for the association to continue the operation under government subsidy). We can speculate on the effects of the transfer on the association's own information program. For example, the American Psychological Association cannot indefinitely continue to produce an abstract journal without steep increase in subscription costs, an abandonment of the principle of comprehensiveness, and increasing delays in processing. Assuming at some point that the government subsumes psychological abstracting under a broad interagency system of behavioral science abstracting, what might be the effect on the APA? We have seen recent innovations in its informal information system, and there is every reason to believe that, as APA gives up its archiving operations, it will replace each with a new informal mechanism. The hypothetical case of the APA illustrates the contention that the political system has an impact on the system of the membership group, the professional association, and that system, in turn, adapts its policies and programs in a way that has an impact on the invisible college. When APA or any other association introduces on a broad scale informal mechanisms that were previously restricted to invisible colleges, the result is a "democratization" of the invisible colleges. Such an innovation as the publication of a list of manuscripts accepted by a journal, together with authors' names and addresses, makes research results available early to an interested audience far beyond the invisible college to which their prepublication circulation was previously restricted.

Invisible Colleges and a Membership System like AERA

The editor of the American Educational Research Journal has for some time been publishing list of accepted manuscripts. What else might AERA do to make some of the advantages of invisible college membership available to thousands instead of dozens of members at a time, without upsetting delicate balances that allow a true invisible college to survive?

What seems to me a logical extension of the best pattern of invisible college interchange is an annual meeting organized around invisible colleges as a fair is organized around special buildings. Various associations have chosen to scrap divisional structures in favor of "special interest groups," which is the name invisible colleges sometimes give themselves when they come out into the open. These associations have, in some cases, taken the additional step of allocating convention time to the special interest groups, just as APA and AERA now allocate time to divisions.

If the special interest groups are indeed that, and not just truncated divisions, then the convention sessions they organize have an informality and freshness reminiscent of an invisible college meeting *ad hoc* on its own ground to discuss its enthusiasms. Convention sessions have earned their deadly reputations because program committees ponder the question, what *should* we cover? SIG's one would hope, ask only, what would be *interesting* to cover? Sessions organized by special interest groups with that attitude might encourage us to take conventions less seriously. There are other information channels meriting a serious approach, but the light touch of the invisible colleges can prevail at annual conventions of a fairly large association only if the entire spirit of the convention shifts several degrees toward the informal.

The special interest group, as a front organization for an invisible college, can also help the professional association to achieve other kinds of informal communication. For example, preprint exchanges among all members of a SIG is one way in which the quick reaction time of the invisible college is exploited. Several information exchange groups and groups around the country organized under other names have experimented with preprint exchanges, often finding that the operation becomes too successful to continue operating off the corner of someone's secretary's desk. When these experiments become successful, they kill themselves off, but similar exchanges arranged by a professional association would be able to survive success.

This discussion of the relationships that might be established between an invisible college and a professional association like AERA has tended to emphasize ways in which values and mechanisms of the invisible college could be generalized throughout the association and not vice versa. The emphasis is deliberate. The two systems in which a researcher invests so much of himself--his work team and his invisible college(s)--have to be studied to discover what rewards in them merit the investment.

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THE ORGANIZATION AND DIFFERENTIATION OF THE SCIENTIFIC COMMUNITY: BASIC DISCIPLINES, APPLIED RESEARCH, AND CONJUNCTIVE DOMAINS

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Introduction

In order to understand the place of educational research in the social system of science, it is necessary first to develop a general picture of the social enterprise that we call science. In discussing the nature of science, I will examine first its epistemological status and the derivative organizational structures, and secondly, focus upon two fundamental dimensions of variation that are applicable across the entire range of scholarly interests, from particle physics to art history. These dimensions are: 1) the continum that runs from "soft" to "hard" and along which different bodies of knowledge within science may be ranged, and 2) the more dichotomous distinction between "basic" and "applied" research.

Once the nature of these dimensions and some of their sociological correlates have been established, I shall turn to a consideration of what I have chosen to call "conjunctive domains" in science--groupings of research activity according to their relevance to broad social concerns. Medical, agricultural, and educational research are examples of conjunctive domains.

Finally, I will discuss in general terms, what I think to be, the most appropriate and effective modes of relating basic and applied interests within conjunctive domains, bearing in mind the relative hardness or softness of these fields and its implications for social organization.

Science and Reality

To the extent that science is concerned with describing empirical reality--with the creation of valid, rigorous, and economical pictures of reality rather than with the provision of practical advice for solving immediate problems--it must begin with the necessary assumption that there is a single reality "out there" which can be discovered through the creative application of intuition, logic, and experiment. The unitary nature of this reality, or its essential coherence, provides an invariant baseline against which the quality of scientists' achievements must ultimately be assessed. Mother Nature, as it were, is the final and totally impartial judge of what is true and what is false in science, and all must turn to her finally for confirmation of their descriptions of reality.

This reality simply exists, no matter what we may say of it, so much of the scientist's energy must go into the construction of a symbolic structure which is capable of describing it correctly. This is a slow, painstaking process, but the importance to Newton of his "fluxions" (or what we call today the calculus), and more recently, the applicability of certain non-Euclidean geometries to problems in basic physical theory, have reaffirmed the point that men must adapt their symbols to reality rather than simply hope that reality will turn out to fit their traditional, everyday symbol-systems.

While all empirical phenomena must be ultimately related or interdependent, in the very long run, there are "natural clusters" of phenomena which seem to make up the most efficient foci of attention for research. These clusters tend to be determined by the nature of our own sensory apparatus. They may be identified by their relation to a particular sensory channel, as for instance the study of light is defined as a "natural" field of study because of its immediacy to our optical sensors. They may be identified because they are composed of phenomena that are characterized by cause and effect chains which are partially independent of other phenomena adjacent to them in time and space. Frequently, such natural foci of attention have tangible spatial characteristics as well as rocks, oceans, and living organisms have physical boundaries, and some things like the moon and the Dead Sea have a unique physical location as well.

With the passage of time, then, we have come to define as suitable topics for investigation those phenomena which satisfy one or more of these criteria, and we have adjusted our symbol-systems to provide the most effective identification and description of these phenomena. Put another way, we have learned slowly to distinguish the objects of our research according to the senses upon which they impinge, their size and location, and their cohesiveness as relatively closed systems of causes and effects. Out of these criteria have come the various subdivisions of scientific interest that we accept today as the basic scientific disciplines.

It may be useful, incidentally, to view current attempts to develop new disciplines or interdisciplinary fields as attempts to restructure our symbol-system so that the foci of their attention can effectively be conceived as making up natural clusters of physical events, even when, by traditional criteria, they are composed of disparate and relatively unrelated phenomena. While the frequent failure of such efforts may be attributed in part to resistance by vested interests--the already established disciplines--it would be a mistake not to recognize, as well, that often their chosen foci of attention do <u>not</u> make up "real" clusters of phenomena, no matter how conceived, so that attempts to build cumulative bodies of knowledge about them are doomed to failure because of the intrinsic structure of reality.

Even granting that radically new ways of conceptualizing the world might lead to the establishment of newer and more powerful ways of organizing collective research efforts, we are still dependent of the linguistic structure that we received as children; the problem of devising and coordinating changes in it is bound to be extremely difficult. At any given point in time, only a little change can be successfully introduced and we must ordinarily be content to work mostly within the symbol-systems that are common to us and our fellows.

Out of this reasoning emerges the conclusion that the separate academic disciplines, as they are now identified, constitute: 1) reasonably valid identifications of the major "natural" clusterings of empirical phenomena, and 2) the major opposition to improvements in the way we might wish to identify researchable clusterings of these phenomena.

As things stand, then, and in the sense that scientific research is essentially the <u>disinterested</u> search for knowledge (uninfluenced by the need to solve immediate, practical problems), we must accept the traditional lines separating the basic disciplines as representing the best we have been able to achieve in identifying suitable areas for the concentration of research efforts. Within the social sciences, the disciplines of psychology, sociology, anthropology, economics, and political science--not to mention geography, linguistics, social psychology, and history--must thus be accepted as the formal categorization of social phenomena that we have found to be most effective over the last century or so.

Such a disciplinary method of organizing the scientific community must be distinguished from that which groups researchers according to their interest in different broad areas of immediate social concerns. Cutting the pie by this criterion yields such conjunctive domains as "agricultural science," "space science," "medical research," and "educational research". Agricultural science includes representatives of the disciplines of entomology, plant physiology, pathology, and meteorology, together with the more practical fields of soil physics, plant breeding, and food processing. Space science includes astrophysics, life support systems, rocketry, celestial navigation, exobiology, and a number of other specialties which, while they do not carve out a single natural cluster of causes and effects, are all relevant to the real-time and real-space problem of transporting men to the moon and back.

There is thus a critical distinction between the basic disciplines--those organized around the investigation of empirical phenomena which seem to hang together regardless of men's individual or collective interests--and those conjunctive domains which are defined so as to include all the phenomena that are involved in a specific social need or problem. This point will come up again later, but for the moment I wish merely to take note of the fact that educational research is a conjunctive domain rather than a scientific discipline. This fact will have important implications for the ways in which educational research can be most effectively organized and upgraded.

We shall return to this after a consideration of the basic dynamics of the scientific community and of the two central dimensions along which different fields of research may be ranged.

Basic Dynamics of the Scientific Community

To talk about dynamics is to talk about both sources of energy and the structure through which it is manifested. Here, we are concerned with the fundamental nature of the "energy" that keeps science going and with the ways in which it is channeled so that scientific activity can continue. Because science is an intellectual activity, we must agree in the beginning that the source of this energy must be motivational--why do scientists want to engage in research and the other activities associated with it? And we must agree that its structuring is thus determined by the special set of norms and values that distinguish science from other sectors of society.

This definition of the problem has provided the framework for much of the "basic" research on science, beginning with Robert K. Merton's pioneering essays in the 1930's and continuing to the present day. This is not the place to go into the history of the sociology of science, but it is of interest to note that what is now conceived as the central "energy" of science was not identified until after the norms that structure its flow had been determined. In 1937 Merton suggested the four basic normative prescriptions which make up the "ethos" of science, (1) a description which has not been seriously challenged since then and in 1957 he presented his analysis of the central energy that underlies scientific activity. (2)

In retrospect, however, it is easier to begin with the nature of this energy, since the character of the goal toward which it is apparently directed has much to do with the nature of the norms that guide it. Merton pointed out in 1957 that professional recognition, the celebration by one's colleagues of one's scientific achievements, is the single most appropriate reward for the scientist. Through the analysis of a series of disputes over priority in scientific discovery, ranging from the controversy between Newton and Leibnitz over the discovery of the calculus to the less sensational but still meaningful resolution of the question whether Darwin or Wallace had first hit upon the theory of evolution, Merton was able to demonstrate that the receipt of professional recognition, earned by discovery, is indeed of central importance to the scientist's motivation. Even if he is reluctant to admit it, the scientist yearns for indications that his work has been accepted by his colleagues as valid and significant; indications that range all the way from being mentioned in a footnote to being awarded a Nobel Prize. This is not to say that all research is done simply in order to gain recognition, but, that without such feedback muct of the desire to engage in research would quickly dwindle.

Why the scientist should want professional recognition is a question that has not yet been fully resolved. Two major hypotheses have been advanced to attempt to explain this. First, there is the proposal that the scientist is <u>trained</u> to want recognition because it certified that he has satisfied the demanding requirements of his role: he has advanced our knowledge of some aspect of reality. (3) A complementary hypothesis, thus far espoused only by myself, so far as I know, contends that the desire to create, to produce "meaningful novelty," is a basic human need and that the act of creation is not complete without the receipt of competent response to it from others. (4) The discovery of a regular relationship between physical phenomena is a type of creativity, especially since the relationship must be described in words or mathematical equations if it is to take its place in a body of knowledge, and the person responsible for it needs the affirmation of relevant others that his creation is valid and meaningful. In science, professional recognition constitutes this positive response to creativity. In other contexts, a person may desire affirmation of the beauty or cleverness or practical utility of what he has created, but the basic need in all forms of creativity seems to be the same.

Regardless of <u>why</u> the scientist desires professional recognition, it is possible now to assert that the desire fo it is the <u>normatively appropriate</u> motive of the scientist, even though specific individuals may find a variety of other rewards for engaging in research. This central assertion is supported by indirect evidence of several types, and the primary objection to it that remains is its apparent conflict with the idea that the scientist is disinterested, altruistic, and entirely unconcerned with fame and fortune.

Again, two different but complementary lanations have been offered for scientists' reluctance to admit ir interest in receiving professional recognition. (The numerou ıcidents of priority conflict, together with other data showing the arge proportion of scientists who admit to an occasional worry abou "being scooped," are sufficient to dispose of the contention tha they <u>don't</u> really care about recognition.) (5) One explanation is that there is another norm in science which calls for humility and works to make the scientist deny his interest in professional approval. (6) The other explanation is that since such recognition is worthless if it is not objective--it is supposed to represent Mother Nature's judgement of the validity and significance of the discove<u>ry</u>, not the discover<u>er</u>. The scientist hesitates to admit his interest in professional recognition because this might lead his colleagues to bestow it as a favor to him rather than as an impersonal evaluation of his work. (7)

With the "energy" behind scientific activity now identified, we can turn to a consideration of the rules which guide it or which direct the relationships among scientists so that collectively they can continue to carry out their research, have it objectively evaluated, and receive enough of the reward of recognition to retain their enthusiasm for their occupation. The four norms which Merton first described concern scientists' relations with each other rather than their attitudes toward empirical phenomena, and upon logical examination they turn out to be the minimal set of directives that can simultaneously promote the cumulative advance of knowledge and sustain the motivation of those engaged in this work.

These norms are actually the sociologist's highly abstract names for distinctive clusters of behavioral preferences, and no claim is made that scientists themselves are directly aware of these norms or that they would give them these names if they were so known.*

The first is universalism, which describes scientists' tendency to assume that natural phenomena (when properly abstracted from their immediate concrete context) are everywhere the same, and also to make a complete distinction between the validity of what another scientist says and his characteristics as a person. Thus, the scientist assumes that what is discovered to be true about X-rays in Germany will be true also in England and in Japan, and he ignores the political, social, and religious characteristics of the discoverer when he is evaluating the research.

The second norm is communality, referring to the complex of behaviors associated with scientists' unwillingness to keep their findings secret or to allow other acientists to do so. Instead, they insist upon absolute freedom of communication, upon defining a new discovery as a "gift" to the entire scientific community. If the norm of universalism works to keep the scientist's attention focused upon the research output rather than upon irrelevant personal characteristics of his colleagues, the norm of communality ensures that his own chances for scientific achievement will not be affected by extra-scientific issues. He is supposed both to have full access to the findings of others and to make his own findings available to them. This norm thus produces a kind of routine, reciprocal generosity that maximizes the speed and effectiveness of scientific advance.

The third norm is organized skepticism. It is perhaps best summed up in the wry statement, "A scientist is a man who takes a quarrelsome interest in his neightbor's work." This norm describes the obligation of every scientist to receive with critical scrutiny each contribution that another scientist makes to knowledge in his own field and to make known his evaluation of it. It requires him also to be just as critical of his own work before he goes ahead to share it with others.

In this way the mutual policing by scientists of each other's work is encouraged, so that only research of high quality is accepted into the corpus of "certified knowledge" that forms a discipline's

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* For a more extensive discussion of these norms, see Storer, Social Systems of Science, pp. 76-86.

body of scientific truth.

The scientist, incidentally, who identifies a flaw or error in another's work, which has presumably been missed by other scientists, is accorded a kind of recognition that seems to differ in quality from that accorded the discoverer of unchallenged truth. Exposing errors is still a meritorious and valued activity, but as it may serve only to expunge from the record a false contribution to knowledge, the critic's name together with that of the error's author may be lost in the annals of science--unless at the same time he has managed to set things straight. The names of Priestly and Lavoisier would be far less familiar today if they had not, at the same time they were demolishing Stahl's theory of "phlogiston," identified correctly the nature of oxidation and thus fundamentally influenced the direction in which chemistry was to advance.

The fourth norm is that of <u>disinterestedness</u>, which is at first simply a description of scientists' ambivalence toward the receipt of professional recognition. Merton originally defined this norm as operating to discourage scientists from seeking recognition openly, but it seems to operate equally as a deterrent against using one's research to acquire any of the rewards that society customarily bestows upon high achievement: wealth, influence, and fame. The norm thus serves both to insulate the scientist against the temptations that society may offer if he will but turn his research to the solution of practical problems, and to keep his interest properly focused upon the reward which his colleagues alone can allocate. He is in this way kept attentive to his colleagues' interests, which represent the "needs" of a growing body of basic knowledge, and thereby motivated to work within the structure of his discipline and sustain its momentum.

These four norms represent ideals or central tendencies in the behavior of scientists, rather than being precise descriptions of the way scientists actually behave all the time. They make possible the continued cooperative functioning of the scientific community and serve as standards by which scientists judge each other's behavior. In combination with the "energy" provided by scientists" quest for knowledge, of which the interest in professional recognition is an intrinsic part, these norms have produced a viable, self-sustaining social system which has been growing steadily over the past 300 years on an international basis. (8)

I have set forth here a bare-bones model of science, existing, as it were, in empty Euclidean space. The body of knowledge which is the product of scientists' research provides the baseline against which future scientific achievements are assessed, and also generates further questions and further opportunities to earn professional recognition through solving them. Beyond the existence of the underlying energy of science and the norms which guide its flow, the scientific community needs only a benign social environment which provides material and moral support for its work, as well as a steady supply of new recruits, in order to flourish.

Lest this description seem so abstract as to be unrealisitic, it must be noted that there are other factors, both internal and external to science, which exert considerable influence upon how effectively the reward system operates and upon the ways in which the norms of science are implemented in specific forms of behavior. The sociologists of science have not yet codified these influences in a concise paradigm, but we can discuss what appear now to be two especially important forms of both internal and external variation in these influences.

The principal source of variation among the different disciplines within science is apparently an aspect of the way the disciplines' bodies of knowledge are organized. This is the distinction between "hard" and "soft" sciences, the topic of the next section.

Hard and Soft Sciences*

"Hard" and "soft" are adjectives in common use when scientific disciplines are being compared. The terms call to mind several possible attributes of these disciplines--the relative difficulty with which the fundamental body of knowledge of one or the other can be mastered, the degree of certainty with which its central propositions have been established, even the relative tangible-ness of its major phenomena. A more meaningful interpretation of the use of these terms, however, requires that we look at a more measureable aspect of the bodies of knowledge to which they are applied. It is generally agreed that physics and chemistry are "hard", while such social sciences as sociology and political science are particularly "soft". These differences seem to be best correlated with the degree of tightness of organization that characterizes the different bodies of knowledge. A "hard" science, then, is one whose store of certified knowledge is largely in quantified form. Its central concepts are identified and their relationships are expressed in mathematical equations rather than in prose. The harder a field is, the more its fundamental understanding of the phenomena with which it is concerned is expressed in quantified form, giving its body of knowledge a tightness of organization that facilitates rigorously systematic organization and makes it readily susceptible to empirical test.

A "soft" science, on the other hand, is characterized by a loosely organized body of knowledge. Its basic concepts are less frequently defined quantitatively in terms of their relationships to each other, and the "channels of implication" among different subclusters of phenomena are vague and uncertain. That is to say, the

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* A more extensive discussion of this topic is presented in Storer, "The Hard Sciences and the Soft: Some Sociological Observations," Bulletin of the Medical Library Association, vol. 55 (Jan. 1967) pp. 75-84.

relevance of one finding for the validity or theoretical implications of another is difficult to determine because no clear-cut, rigorous statement of the relationship between these two more general phenomena exists. If it were found, for instance, that poor people are generally happier than rich people, it would be almost impossible for sociologists to determine the logical consequences of this discovery for the theory of social stratification.

There may indeed be small "islands" of hard knowledge within a soft body of disciplinary knowledge--just as there are undoubtedly some soft areas within the harder sciences--but if these well-quantified islands are not rigorously related to each other, the essential attributes of softness remain.

The relative hardness and softness of different bodies of knowledge has important implications for the relationships among the scientists working in these different disciplines. Because it is easier to ascertain whose work is directly relevant to one's own in a harder field, such fields are characterized by both greater efficiency (in the sense that progress is more rapid) and a greater amount of competition for professional recognition. To cite an obvious example, it would be very difficult to find an example of a race in sociology that would be comparable to the race among biochemists to determine the structure of DNA as James Watson has described it. (9) A softer field simply lacks the guideposts that identify the problem which is most in need of solution, partly because it is difficult to tell which problem's solution will be of the most strategic significance for work on other problems, and partly because there is no firm concensus among the scientists in a softer field regarding even the criteria by which significance is to be determined.

A second and equally important difference between "hard" and "soft" sciences is the greater ability of the former to allocate professional recognition quickly, fairly, with relative permanence. Lacking criteria of significance, and sometimes even validity, the softer sciences are without the standards by which professional recognition can be so effectively distributed. Instead, "schools" spring up, each with its own local standards of significance and validity, and they compete vigorously for recognition, recruits, suitable academic positions, and intellectual dominance within the larger disciplines. (10)

The degree to which a body of knowledge is "hard" is thus of crucial importance for the way a field of science is organized. The pyramid of stratification according to scientific accomplishment is steeper and more clearly defined within a hard science; its outstanding scientists are recognized and appropriately rewarded; and other have less reason to feel that they have been unfairly relegated to the lower levels of this pyramid. The less talented members of the discipline may become discouraged, but the game is obviously played according to a hard but fair set of rules, (11) and one's own lack of talent must be blamed for their failure to reach the top ranks within the profession.

In contrast, the stratification pyramid in a softer science is likely to be broader and lower, affording a greater proportion of its members the chance to feel themselves reasonably successful and yet at the same time rendering their credentials more suspect. The "currency" of professional recognition undergoes more inflation and deflation in a soft science, so that reputations are more fragile and there is less conviction that the processes through which it is acquired are completely fair. Failure to succeed in a soft science may more often be blamed on the "politics" of the discipline than it is in a harder science.

Such a situation is naturally unsatisfying to the members of a soft discipline, and efforts are made to rectify it. For instance, methodology receives more attention, for it is assumed that methodological advance will provide surer grounds for assessing the empirical validity of research findings, even if they do not aid directly in establishing a more deterministic structure for the accumulated knowledge of the field. In the early 17th century, when what we now think of as the hard sciences were getting started, Francis Bacon paid a considerable amount of attention to the philosophy amd methodology of science, and perhaps the methodological asides, called "scholia", that Newton inserted at various points in the Principia, illustrate the same thing. Claude Bernard's writings in the 19th century on the methodology of physiological research seem to exemplify the same concerns at the beginning of that discipline's development. Today, of course, methodology is an important concern in the social sciences.

Efforts are also made to increase the degree of quantification in the way that relationships among phenomena are described. These efforts may stem partly from the simple desire to emulate the harder, more successful sciences, but even when the attempt is premature (as in the application of rigorous tests of significance to sociological data that are of extremely uneven quality) the basic reason for wanting to emulate a field like physics is valid. In the long run, quantification must be the major path to both scientific success and the foundation of satisfactory social organization of a discipline, even though its superficial trappings do little to cure the central weaknesses that are inherent in a poorly-organized body of knowledge.

The development of "hardness" is apparently as much a function of time as it is of creative brillance. It requires the forging of concensus on standards of proof and the rigorous definition of central concepts. This process cannot be speeded up because it involves persuading a field's most mediocre members as well as its most highly talented to accept these changes. Probably a minimum of a generation of scientists is needed to make a significant advance in hardness, and movement from one end of this continuum to the other may well require three or four generations.

The treatment here of this type of internal variation among scientific disciplines seems to me an extension of the assertion made earlier that there is only one effective strategy in our approach to the investigation of empirical reality. Once natural clusters of phenomena have been identified, the next task is to develop a set of symbols which can most effectively describe their relationships. As greater quantification is achieved in this process, a discipline becomes more effectively organized as a social entity because it is more successful in its investigation of reality. Disciplines may thus have something comparable to Rostow's "take-off point" in economic development, and even if we cannot do much to help a soft field reach this point sooner, we can at least visualize it as the main goal and thus avoid some pitfalls on the way.

Basic and Applied Research

In describing research as basic or applied we are dealing with a dimension of variation within the scientific community that cross-cuts the hard-soft dimension. It concerns different motivations within the discipline rather than difference among them. In terms of its implications, the distinction between basic and applied research is as important as that between hard and soft disciplines, any may be even more relevant to the successful activities of an organization concerned with a conjunctive domain.

Applied research is ordinarily thought of as research that is intended to help solve a practical problem, while basic research aims toward the extension of a body of generalized, fundamental knowledge without regard for the utility of its findings. In principle, of course, the ultimate utility of research findings can be determined neither by the researcher's motives nor by the needs of those who support his research, and if the terms basic and applied are to be defined strictly in terms of actual utility, it is impossible to categorize research projects while they are going on. Yet scientists do classify research as basic or applied while it is in progress; apparently, the distinction has symbolic if not utilitarian meaning for them.

Scientists seem to make the distinction on the basis of the researcher's apparent motivation, which we may think of as a kind of "audience" for his findings that the researcher has in mind. The basic scientist is presumably trying to carry out research which will be of interest to the other members of his discipline. He selects for his topic a problem whose solution will be of value in extending his discipline's body of knowledge, and thereby seeks the reward

* A more detailed consideration of this topic is presented in Storer, Social System of Science, pp. 106-115.

that only his colleagues can bestow, professional recognition. The applied scientist, on the other hand, selects his research problem (or has it selected for him) because of its relevance to the solution of an immediate, practical problem. It will be only coincidental if his findings turn out to be of interest to his colleagues, for the audience he seems to have in mind is composed largely of nonscientists--his employers and/or the general public. And since this audience is not, by definition, competent to provide meaningful recognition as the reward for successful performance, the scientist must accept, instead, the reward which is ordinarily used elsewhere in society: money.

The basic scientist's monetary income is usually received in return for the teaching that he does in a college or university, and the focus of his research is thus unrelated to the source of his salary. He may even receive a salary solely on the grounds that whatever basic research he chooses to do will be important. The applied scientist, however, receives his salary specifically because of the particular research he is doing. The money and research support he is given are treated as an investment which should generate future profits if he works in industry, or as part of the costs of providing services to the public if he is employed by the government. For this reason he is not free to select the topic he will investigate solely on the basis of its importance to a growing body of scientific knowledge.

Under these conditions the applied scientist presents a "threat" to basic science. He is unable to contribute much to the general advancem[illegible]ledge in his discipline because his research is guided [illegible]iderations. He is presumably less motivated by the des[illegible]quire professional recognition than a scientist really "should" be. Finally, he is probably less able to <u>give</u> professional recognition to others because the constraints governing his research tend to keep his attention focused on problems other than those of the basic discipline, so he is less able to recognize significant work in the discipline with which he is identified. Because of his apparent defection from the central ethos of science and his presumed inability to participate in the central concerns of his discipline, the applied scientist becomes the victim of a pervasively invidious distinction between basic and applied science. Because the former is accepted as more prestigious, more desirable and intellectually demanding, he assumes a kind of second-class citizenship in science as a result of the context of his research, regardless of its quality.

We are of course dealing here with matters that are to some extent myths, and with scientists' reactions to them, so that examples of these generalizations about the relations between basic and applied research are not always directly observable. Yet the fact that the applied scientist (generally identified by <u>where</u> he works and <u>what</u> he works on) is in principle violating some of the basic norms of science means that these myths have at least some foundation in fact.

The relegation of the applied scientist to a lower status within the stratification system of science can be seen as a form of self-defense on the part of the basic research community, and most applied scientists seem to recognize the partial legitimacy of this situation.

The conflict between basic and applied science is intrinsic to the nature of science, then, and cannot be resolved, even though, in some instances, men are able to surmount or ignore it and either combine the two types of research or else move back and forth between them without apparent strain. But so long as basic science proceeds by abstracting natural clusters of phenomena out of concrete situations and studying them in isolation from the other factors that are present (as two chemicals may be purified and isolated in a test tube in order to observe the reaction), the directions in which basic science tends to develop are bound to be markedly different from those in which knowledge relevant to the solution of practial problems must develop.

The makeup of a concrete problem, as for instance the need to improve the educational process in a ghetto school, is not at all correlated with the most efficient categorization of the various different phenomena that are involved in the concrete situation. Such conceptually discrete topics as cognitive processes, linguistics, small groups, demography, the economics of public services, the political science of municipal governments, the sociology of the processes through which people come to be teachers, even the history of the local community--all are implicated in the situation, and knowledge of each will be relevant to the concrete problem of improving a particular ghetto school. Yet the basic social science disciplines abstract each of these components of the situation from its immediate cont xt in order to understand it more clearly, and it seems too much to expect that any one person can know enough of these components--let alone draw up a coherent picture of their interrelationships-- to produce an integrated, theory-based plan for improving the situation. Applied research on the same problem must employ a different set of priorities: what is of practical importance, rather than what is theoretically significant, what is politically and economically feasible, rather than what is the nature of a single facet of the situation? It is the relationship between total input and practical output rather than the detailed relationship between a carefully administered input and an isolated, theoretically significant output, that is the focus of the applied researcher's attention.

To restructure the basic scientific disciplines so that they concentrate on concrete problems rather than abstract, natural clusters of phenomena would destroy their ability to develop cumulative, integrated bodies of knowledge. On the other hand, to give up those groupings of researchers organized around practical topics--agriculture, space, the cities, education, etc.--would be to lose not only the accumulated practical expertise they have developed, but the opportunity for men to concentrate full-time on these topics. It should be noted, incidentally, that success in solving practical problems is not necessarily dependent upon the existence of a public, integrated, empirically tested body of knowledge; it is only when a practical problem can be broken down into a set of technical problems, as in the development of a compact nuclear power generator, that cumulative bodies of abstract knowledge become relevant to its solution.

Both modes of organizing research will continue to be employed, then, and they will apparently continue to be related more by chance and ad hoc collaboration than by the systematic integration of their distinctive bodies of knowledge. The tendency to draw invidious distinctions between them will continue to exist also, even though the advantages to be gained from even sporadic efforts at cooperation will make it impossible for them to sever relations altogether. Basic research interests will continue to profit from the stimulation that close acquaintance with practical problems can provide, and applied research will continue to draw upon the generalized paradigms that have been developed previously by basic research.

The inherent conflict between extending a body of knowledge and utilizing it, however, seems to be built into the very foundation of the scientific profession, and the problems we face in harnessing the two modes of research are only now beginning to come clearly into focus.

Educational Research and the Social System of Science

Having discussed the two principal dimensions along which the scientific community is differentiated, we are now in a position to examine the place of educational research in this picture.

The very name of the field suggests that it is applied rather than basic, in the sense that its focus of interest as a whole is concrete rather than abstract, and is of direct concer[illegible]lic as well as to the researchers themselves. I would arg[illegible]hough, that it is one step further removed from the concept of a basic scientific discipline that is something like applied physics because it encompasses both the basic *and* applied interests of a number of disciplines. Educational research is not so much an applied science as what is called at the beginning of this paper a *conjunctive domain*, meaning that its focus is a socially-relevant whole rather than a natural cluster of abstract phenomena. Different aspects of education are of legitimate interest to basic researchers in a large number of social science disciplines, and because those areas are too soft to have developed distinctive applied-research interests within them, basic research questions tend to dominate the field at present.

Educational researchers thus tend to abstract from the concrete educational situation those particular phenomena which are of interest to them, and to report their findings in terms which are primarily meaningful to their colleagues rather than to those concerned with the practical problems of education. Yet as more of these people come to be housed within schools of education, and as they sense a growing "conceptual distance" between their work and that of their colleagues back in their "home" disciplines, the fact that they share with others a single generic term to denote their interests may lead them to believe that they should have more in common with each other than with their disciplinary colleagues.

It cannot be doubted that these researchers share a common enthusiasm and look forward to building a coherent body of generalized knowledge about education, but I must confess to considerable pessimism about the chances for success in this direction. Given the vast range of disparate social phenomena which come together in the concrete social enterprise called education, and the present degree of softness in the social science disciplines, I think it highly unlikely that educational research can ever be developed in such a way as to enable its adherents to enjoy the advantages that characterize the hard, basic scientific disciplines. It may be virtually impossible to build a tightly-integrated body of knowledge about the concrete process called education, even though such a structure would be necessary if researchers in this area are ever to be able to build upon each other's work and to participate in the exchange of professional recognition throughout the field.

This is of course a long-range point of view and assumes that a "genuine" discipline must be held together by <u>internal</u> forces. That is to say, a discipline focuses on an intrinsically interrelated set of phenomena and provides opportunities for its members to cooperate in building a cumulative body of knowledge and thus to participate collectively in the giving and receiving of professional recognition. Over the short run, on the other hand, it is true that something <u>looking</u> like a discipline can be established if there is sufficient support from outside to take the place of the centripetal forces found in a genuine discipline. If researchers are attracted to a topic because it is of practical importance (so that the public can appreciate one's achievements) and because there is ample support available for such research, they may adopt a common identity and behave organizationally like members of any other discipline. But the likelihood that such a "discipline" can ever develop an organized, cumulative body of knowledge is still quite remote, and as soon as outside support for it declines its members will tend to return again to their basic disciplines.

It might be argued, I suppose, that it is only historical accident that the disciplines are organized as they are today, and that with a different tradition of cutting the pie of empirical phenomena we might now have departments of educational research that were every bit as "successful" as are today's departments of zoology and history. But I suggest that the matter is not so arbitrary. If there is only one reality "out there", toward which there is only one effective strategy of conceptualization and research, mankind's collective cut-and-try efforts over the past three centuries have given us a reasonably close approximation to this strategy. It entails grouping men's efforts so as to parallel the natural clusterings of interrelated phenomena that were discussed earlier. This requires that these phenomena be abstracted from the welter of everyday experience for investigation--and at the same time renders the results of these investigations less directly applicable to the solution of concrete problems.

According to this interpretation, it will be impossible to establish educational research as a social science discipline, or even as a specialty within a particular discipline. Because of the heterogeneity of research interests that are grouped under this heading, a viable community of scientists who call themselves "educational researchers" and who relate to each other in the same fashion that biochemists or economists do, is in my opinion simply not in the cards.

In the concluding section, I will discuss what I believe to be the optimal ways in which an organization devoted to a conjunctive domain can relate itself to both the basic and applied research interests that focus on this domain, and can work to make research in this domain as practically useful as possible.

The Conjunctive Domain Organization's Proper Role

It was pointed out earlier that at the level of organized bodies of knowledge and the social structures associated with them basic and applied interests are like oil and water. It is impossible to hold them in a stable mixture. The logic of their separate reward-systems leads them to drift apart and to develop defenses against the threat that each poses to the other. Yet it is important that basic and applied researchers have at least occasional contact for the sake of the benefits that each can obtain from the other.

If my analysis in the preceding section is correct, it follows that an organization devoted to a conjunctive domain cannot expect to function properly at the same level as do basic and applied interests within specific scientific disciplines. By the nature of its concerns, it can operate best as a coordinating structure, mediating the relations between basic and applied interests in a given discipline and among different disciplines. By serving as a kind of neutral ground, it can avoid the implicit tendencies toward conflict between representatives of its component interests and thereby facilitate communication among them. This can lead both to more rapid progress in the different disciplines and to more effective application of their findings to the concrete problems in its domain.

Perhaps an analog to the situation of the conjunctive domain organization is the company which supports basic research in the belief that this will benefit the company in the long run. It keeps an eye on the output of its basic researchers and moves in with other personnel to exploit their findings whenever these appear relevant to the company's practical concerns. It provides a flow of information about its operations to these researchers in hopes of bringing new phenomena to their attention or helping them to see and explain how their work might be of direct value to the company.

But this analogy cannot be carried very far, since the conjunctive domain organization tends to be the creation of its practicing members rather than of outside practical interests who hope to benefit from its activities. It must therefore plan its activities to promote its members' interests, even when these are not likely to result in practical achievements right away. It must devote its energies to helping its members achieve their own goals--sometimes within their own disciplines, sometimes in the area of practical problem-solving--and accept the fact that its future cannot realistically lie in the establishment of a legitimate scientific discipline.

Some general ideas can be offered about the ways in which a conjunctive domain organization can most satisfactorily meet the needs of its various constituent groups. We can assume that these include basic and applied researchers within a number of scientific disciplines, persons who have some public responsibility for solving problems in this domain, and those who are themselves participants <u>in</u> the domain. The relative weight that each of these groups carries within the organization will determine to some extent the priorities placed on its various activities--but this is a political-administrative problem rather than one to which these remarks should be addressed.

Principal among the organization's activities must be the provision of special occassions--conferences, colloquia, consultations, etc.--at which basic and applied researchers can communicate within and across disciplines with regard to specific topics in the general domain. Annual conventions, of course, provide the organization's members an opportunity to participate in a standard sort of scientific activity that is meaningful to colleagues at their home institutions, but these tend to be organized around a congeries of separate topics and their main function is to reinforce the social solidarity of the organization's members rather than to promote the intellectual cohesion of the domain itself.

By establishing committees which can maintain surveillance of research in a number of areas within the domain, the organization can aid in identifying and publicizing the specific research problems that seem likely to be of basic theoretical significance to one or more disciplines and also those which appear to be currently most important to the solution of practical problems. It can provide opportunities for its members to earn professional recognition through publishing in its journals, and it can bring together individuals with practical questions and researchers whose expertise seems most relevant to solving these questions. And, acting as an intermediary between private foundations or public agencies, and its members, the organization can perhaps offer research funds and graduate fellowships for work on topics within its sphere of interest.

To a certain extent, the organization can provide a "second home" for its members, even though it cannot expect to become a complete substitute for their original disciplinary identities. Its central problem will always be that of having members whose loyalties are divided between a conjunctive domain and a discipline, and its sense of its own scholarly legitimacy will have to depend as much on the appreciation of nonscientists for its services as on pride in the achievements of its members which it has facilitated. Yet the conjunctive domain organization is genuinely needed by all of its constituent members, and there can be no doubt that as it defines its role in a way consistent with the underlying social dynamics of science it can serve these needs in ways that are of the greatest benefit to all.

FOOTNOTES

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EDUCATIONAL RESEARCHERS, SOCIAL SCIENTISTS, AND SCHOOL PROFESSIONALS*

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Among the problems confronting leaders of educational research are barriers between basic researchers in the behavioral sciences and applied researchers in schools of education. I should like to approach these problems by considering some of the differences between individuals involved in the social sciences and those in educational research. First, I shall show how educational researchers and social scientists--particularly sociologists, although much of what I have to say can be extended to other social sciences--differ in terms of social backgrounds, university activities, and typical relationships to practitioners. These differences contribute to different ideological stances, about which I shall briefly speculate. Then, I shall discuss how these differences tend to inhibit the flow of ideas and people between the fields and reduce the likelihood of cooperation between them. Finally, I shall point out some of the dilemmas leaders of scientific associations must face if they act to improve the situation.

I. SOCIOLOGY VERSUS EDUCATIONAL RESEARCH

Academic disciplines in American universities are quite heterogeneous in terms of social origins, and it is difficult and dangerous to conclude that differences in the behavior of persons in different disciplines are a result of dissimilarities in origins. Just the same, one may note some of the differences between the social sciences and the educational fields. In the Spring of 1963, 39 percent of the social scientists in American universities were sons of fathers who had attended college, as against 28 percent of those in educational fields. (1) Teaching has traditionally been one of the professions constituting a channel of upward mobility for those from lower status origins, and this is evidently true as well of teaching in schools of education. In fact, most of those in schools of education in 1963 had had professional experience in the schools: 52 percent in elementary and 67 percent in secondary schools. A sizeable but smaller proportion of social scientists, 25 percent, also had had secondary school teaching experience. Professors in schools of education thus came to decide

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on college teaching careers at a relatively late age. They also decided upon specific fields within education relatively late--almost half chose their specific fields after graduation from college, and this is true of about two thirds of the educational psychologists and persons in the "social foundations" area. About half the sociologists also made their career choices this late.

Professors in schools of education are quite likely to have changed specific fields after receiving their doctorates, while for social scientists this change occurs much less frequently. Finally, it is well known that the departments in schools of education, if indeed they are formally recognized, and often heterogeneous in terms of the specific scholarly disciplines of their members, while social science departments are more homogeneous. Thus, compared to educational researchers, social scientists are more likely to have careers in a single occupation and a single discipline, and they are more likely to work with others in the same discipline. It is reasonable to suppose that as a consequence social scientists are more likely than educational researchers to have their primary reference groups composed of disciplinary colleagues.*

An Empirical Classification of Academic Disciplines

The ordinary work activities of professors in schools of education link them more directly to practitioners in the larger society than do the work activities of social scientists. The extent of this can be measured in several ways; I shall indicate it by noting where the various fields are located in an empirical classification of academic disciplines in terms of the characteristics of their members. Disciplines are commonly classified in terms of their social functions quite apart from content. I believe that the most important axes of differentiation are four. First is the amount of research professors ordinarily do; today this means the amount of external research support they receive. Second, research support and research lead to cosmopolitanism and mobility; the greater the research support received in a field, the higher the rates of turnover and job mobility in that field.**

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*The importance of the disciplines as a reference group can be expected to be still greater in such established disciplines as mathematics, physics, and zoology.

**Using the data in Dunham et al., op. cit., for 72 fine fields, I regressed the proportion in the disciplines who were at the same university in 1963 as in 1962 on four independent variables with the following results:

Variable	Standardization Regression Coefficient
Proportion male	.08
Proportion less than 40 years of age	-.18
Proportion whose main activity was teaching	.22
Proportion with outside research support	-.65

The coefficient of determination, R(2), was 0.71.

Third, fields vary in the amount of teaching service expected from them. Some fields almost exclusively teach students majoring in the field itself, while others, such as mathematics and English, teach mostly basic courses to students who will major in other fields, Ordinarily, the greater the emphasis on teaching service, the larger the field, and the greater the proportion of full-time teachers in the field. The fourth dimension is the degree to which workers in the field do consulting work outside the university. It seems to make a considerable difference whether this outside consulting is done for private profit-making organizations, in which case the consulting is done for pay and is associated with job opportunities in industry,* of whether the consulting is done not for pay and for nonprofit agencies.

My own factor analysis of some published data for academic disciplines in the United States in 1963 yields dimensions somewhat in accord with those just described. The factor loadings are shown in Table 1, and factor scores for some of the 72 fields included, together with scores on major indicators of the first two factors, are shown in Table 2. A plot of these indicators of teaching emphasis and research support is shown in Figure 1. The first two factors describe the extent of research emphasis and the importance of teaching service; the importance and extent of extramural service and consulting were not adequately represented in the set of variable analyzed.

Some of the differences between educational fields and the social sciences are indicated in the plot. In 1963 social science disciplines were more likely to be high on the research support dimension than fields within education. For example, 37 percent of the sociologists reported getting research grants in that year as against 15 percent of the faculty in education departments. This was linked to the greater mobility of social scientists: 17 percent of the sociologists moved between 1962 and 1963 from one academic institution to another, while only 5 percent of the faculty of schools of education did so. (The somewhat higher proportion of women in education also contributed to this difference in mobility.) The educational fields are lowest of all on the second factor, related to teaching service. More than their peers in other professional fields, faculty members in schools of education are devoted to the instruction of advanced students majoring in their fields. Thus, while 20 percent of the sociologists reported that they taught mostly freshmen and sophomores, only 11 percent of those in educational fields could report the same; among the latter,

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*Using the data in Dunham et al., Teaching Faculty in Universities and Four-Year Colleges: Spring 1963, for 19 broad fields of science and scholarship, I computed the product-moment correlation between the proportion of the field doing some outside consulting for pay in 1962-1963 and the median academic year university salary at that time; this zero-order r equals +0.75.

Table 2. INDICATORS OF TEACHING SERVICE AND LACK OF SUPPORT FOR 3 SELECTED FIELDS

Factor scores for all 72 fine fields have means of zero, variances of one.

	Factor I TEACHING SERVICE	% teaching freshmen & sophomores mostly	Factor II LACK OF RESEARCH SUPPORT	% with outside research support
1. Educational Psychology	-1.35	25%	.57	27%
2. Elementary Education	-.49	14	1.17	11
3. Social Foundations (History, Sociology, Philosophy)	-2.02	16	1.17	5
4. Industrial Arts	.74	41	1.04	7
5. Secondary Education	-.69	8	.58	14
6. Educational Administration	-2.40	3	.52	16
7. Education: Guidance & Counseling	-1.87	11	.63	9
8. Physical Education	1.49	67	1.13	4
9. Economics	-.30	30	.27	23
10. History	.92	60	.57	8
11. Political Science	-.06	39	.54	21
12. Sociology	-.57	31	.02	37
13. Anthropology	-.50	39	-.08	42
14. Social Work	-1.55	4	.56	25
15. Clinical Psychology	-1.45	13	-.15	40
16. Psychology: Counseling	-2.1-	14	.87	15
17. Experimental Psychology	-.40	33	-.91	62
18. Social Psychology	-1.16	26	.88	48
19. Philosophy	.17	41	.67	8
20. English and Literature	1.43	65	.96	5
21. Art	2.09	48	.98	8
22. Classics	1.12	55	.25	8
23. French	.98	68	1.44	5
24. Russian	-.21	58	1.32	20
25. Mathematics	1.23	54	.10	18
26. Chemistry	1.48	54	-.97	48
27. Physics	.86	54	-1.00	49
28. Geology	.49	44	-.67	58
29. Biochemistry	-.57	8	-2.29	84
30. Zoology	1.17	59	-.57	42
31. Genetics	-.20	14	-1.95	64
32. Pharmacology	-.93	9	-1.74	84
33. Engineering Fields	*	20	*	38
34. Agricultural Fields	*	25	*	70
35. Business and Commerce Fields	*	28	*	11
36. Medicine	.07	4	-.85	76

* Factor scores not computed for broad fields.

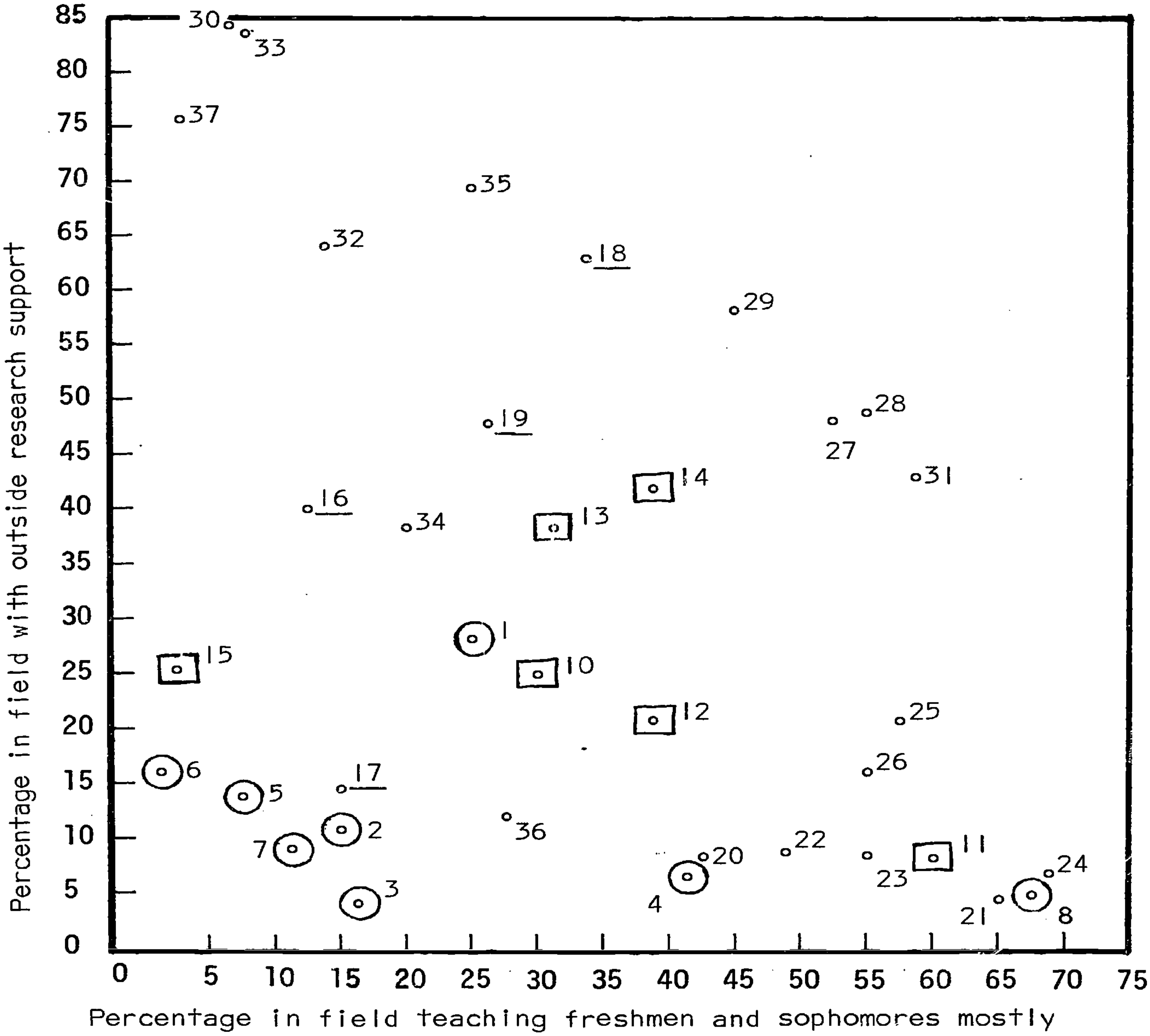

FIGURE 1. PERCENTAGE TEACHING MOSTLY FRESHMEN AND SOPHOMORES BY LEVEL OF RESEARCH SUPPORT, 37 SELECTED FIELDS IN U. S. UNIVERSITIES, 1963

Source: Table 2. Numbers identifying fields are from Table 2. Circles identify educational fields. Squares identify social sciences. Numbers for psychology fields are underlined.

36 percent taught mostly graduate students, while only 20 percent of the sociologists did so.

The differences among the several fields within education are worthy of notice. Educational psychology is closer than the other educational fields to the social sciences in most of the variables treated here, while the "social, historical, and philosophical foundations of education" is farther from the social sciences than most others. Possibily the social foundations area has not provided the bridge between sociology and education that educational psychology has provided between psychology and education.

Social scientists are less likely than professors in schools of education to engage in extramural consulting or service work. In 1963, 49 percent of faculty members in the education fields did institution connected public service and 22 percent did outside consulting for pay; the corresponding figures for the social sciences are 35 percent and 17 percent.

These results show the closer articulation of educational researchers with practitioners and the separation of social scientists from practitioners. Through their students and their consulting, educational researchers are linked with the public school systems, and in 1963 the patterns of extramural research support indicated that they were not likely to weaken this link. Social scientists, on the other hand, are seldom aware of preparing students for particular professional positions. While more than one third of the undergraduate majors in sociology are likely to become social workers or secondary school teachers, this is not at all a salient factor in the work of sociologists. In sum, practitioners can be expected to be a powerful reference group for educational researchers, but will be of practially no importance as a reference group for social scientists.

Typical Ideological Positions

While we have some studies of the attitudes of academic faculties to related workers in the larger community, more such studies are needed. My speculations about typical ideological positions are based on casual observations and the proposition that disciplinary colleagues are the most important reference group for social scientists, while educational practitioners are a powerful reference group for educational researchers.

I believe that educational researchers can be expected to have an ideology of service to the schools, a kind of service emphasizing the provision of immediately useful prescriptions and political defense. Consider political defense first. The public school system in the United States in vulnerable to political attacks from a wide variety of sources. Organizations representing class groups, ethnic and/or religious interests, professional and industrial groups attempt to induce the schools to adopt some programs and policies and abandon others. School professionals have generally dealt with such threats by stressing the universalistic achievement aspects of the system: common goals are emphasized, and the possibility that groups in the larger community may have incompatible conflicting interests is denied. Educational researchers, along with other professionals in schools of education, participate in this type of response. Thus, it took a major social upheaval to generate much interest in the effects of racial conflicts on education (and even now the defensive reactions are manifested in the euphemisms used for racial groups and racial conflict), and such problems as religious conflict are still taboo. The denial of conflicting interests, so well manifested in greatly elaborated and seemingly vacuous discussions of educatin "goals," irritates sociologists, whose business it is to study social conflict, as well as other academics, who can self-righteously express the interests of some clients of the schools simply because they have no sympathy for politically vulnerable educational professionals.

Any healthy applied science must have an ideology of service, but such ideologies are compatible with quite different images of clients. The assumption that the goals of the schools are unambiguous and known may lead educational researchers to develop an image of an "unqualified" practitioner. Given alternative procedures for attaining a single clearcut goal, the researcher can prescribe the "best" procedure without paying much attention to its costs in terms of achievement of other goals. The prescription of the researchers is supported by evidence and arguments that the teacher or school administrator cannot gather or construct by himself. School administrators often seek such directions that seem authoritatively based: they aren't interested in oracular or ambiguous advice. But, when different researchers offer different advice, or when the recommendations are difficult to implement, administrators find it quite easy to ignore the prescriptions they receive. One of the major problems of educational researchers is to find ways of inducing practitioners to implement their suggestions. But, researchers must first develop a consensus among themselves and conduct research studies of large scale that will be almost definitive; when there is "dissensus", or recommendations are based on studies of small and unreliable populations made with unreliable techniques of measurement, it is no wonder that practitioners are skeptical. It also goes without saying that, if educational researchers express alienation from or contempt of school professionals their prescriptions are less likely to be accepted. (It has been noted that faculties of elite graduate schools of education frequently have no influence on the large school systems in their own urban areas.)

Let us turn now to the relevant aspects of the ideologies of sociologists. Sociologists are likely to take pride in developing an objective, value free basic science. It is true just the same that most, and probably the best of their work is value relevant: (2) they investigate topics about which members of the larger communities, and sociologists themselves, have strong values. More than educational researchers, sociologists are likely to hold or to have held radical political convictions, and they may have political reference groups as well as disciplinary ones. However, the value relevance of sociological research seldom leads to prescriptions for practice. Instead, the implications may be to criticize existing social practices, to discover their latent functions or disfunctions, or to point up dilemmas for practical actors. It is perhaps the distinctive role of the sociologist to discover the latent functions of social practices, those unrecognized consequences of action that may help maintain or may weaken the stability of an ongoing system.

These characteristics of sociological work can be illustrated by studies in the area of education. Studies of social class and education often give rise to criticism. Hollingshead's Elmtown's Youth (3) reports what might be called a series of atrocity stories, although moral evaluations are left unstated. The element of social criticism is much more apparent in the later work of Patricia Sexton. (4) Edgar Friedenberg is a rather deviant sociologist whose criticisms of social practices are exemplified by Burton Clark's work on junior colleges, (6) in which many junior college practices are interpreted as ways of reducing the level of aspiration of students, and by various explanations of parent-teacher associations as means by which parents are coopted by the administrators of schools systems. (7)

Becoming aware of the latent functions of social practice means becoming aware that the practices are relevant to multiple goals that are not always consistent; for the practical actor, this means becoming aware of dilemmas. Other kinds of sociological work also spell out dilemmas for actors. Neal Goss' study of role conflicts of school superintendents clearly shows that the conflicts, the dilemmas, are inescapable, and Gross provides no clear prescriptions for choice. (8) Arthur Stinchcombe's study of rebellious high school students is similar, showing as it does that student rebellion follows from the schools' emphasizing society's social class values and age grading values; Stinchcombe suggests, in very pessimistic passages, that the elimination of the causes of student rebellion would require compromising other values to which school professionals are deeply committed. (9)

These types of sociological research do not yield clear prescriptions for school professionals. I suggest that the sociologists, instead of relating to practitioners as a supplier of useful dicta, tends to relate to them as either a critic or as a kind of dialectical consultant. As critic, the sociologist is alienated from the practitioner; if he wishes to promote beneficial change he does so by stimulating agencies external to a system to force changes upon it. Thus, it is often dangerous to invite sociologists to investigate ones group, whether the group be a local community, a business organization, a police force, or a school system; the results of the investigation may be an exposé. On the other hand, the sympathetic non-alienated sociological investigator may produce not prescriptions but dilemmas; at best he may describe the costs of any of several alternative actions. Unlike the educational researcher, the sympathetic sociologist has an image of the practitioner as one qualified to make decisions; <u>only</u> the practitioner can choose among his values, and the researcher can only help him to clarify these values and the conditions of choice. (10)

I have been presenting ideal typical descriptions of ideological positions. Obviously not all sociologists take the stances I have sketched, and some make clearcut prescriptions to school administrators for changes. (11) Typical ideological positions in the other social and behavioral sciences may be more or less similar to the positions I have described. Economists have contributed very little to the study of education, but their contributions are more likely to be in the nature of clarifying the costs of various alternatives than in making specific prescriptions. Psychologists are a larger and more diverse group; some, especially among the clinical psychologists and certain kinds of learning theorists, might be very similar to educational psychologists. Others are more like biological scientists, indifferent to the potential value relevance of their work and not at all oriented to formulating immediately useful procedures.

II. BARRIERS TO THE FLOW OF IDEAS AND MEN BETWEEN SOCIAL SCIENCE AND EDUCATIONAL RESEARCH

Social scientists and educational researchers tend to have different backgrounds, different working conditions, and different ideological stances toward practitioners. These differences might constitute barriers to the flow of ideas and persons between the two fields, but research is needed to demonstrate that these barriers actually exist. The flow of ideas between fields can be evaluated by citation studies: to what extent do educational researchers cite publications in social science journals and vice versa? Has the rate changed in recent years? These questions could be investigated rather easily. My impression is that sociologists of education tend to be relatively ignorant of work published in educational journals that is relevant to their own research, while there may be rather rapid diffusion of ideas in the other direction.

It should also be possible to evaluate the movement of people between fields relatively easily. We know that about seven percent of those who received doctorates in sociology in the last generation have switched their fields to education and that about four percent of those who received doctorates in education have switched their fields to sociology, (12) but we don't know what kinds of people made the switch. Studies of mobility between fields must take into account vertical mobility between institutions of more or less prestige and must attempt to measure the relative abilities of those switching. Thus, if those changing fields are disproportionately likely to be unproductive, to have received doctorates from low quality institutions, their impact on the conduct of research might be slight. We also know little about the social conditions that might increase rates of mobility between fields. One might hypothesize that the most effective way of inducing sociologists to conduct research on educational topics would be to restrict their job opportunities in sociology departments, but it would be difficult to gain acquiescence for such policy shift.* However, the movement of talented social scientists into education might have been increased by the development of first-rate departments in the social sciences in the schools of education of leading universities.

III SCIENTIFIC SOCIETIES AND SCIENCE POLICIES

Scientific societies may help shape the conduct of research by their control over communications, by their influence on research funding agencies, by their influence on universities and other organizations conducting research. The societies usually have considerable control over communications media; they may have little formal influence on funding agencies, although leading members may have much more authority; and they ordinarily have little influence on organizations conducting research.

By virtue of control over journals and professional meetings, the leaders of scientific societies can encourage or discourage the conduct of various types of research. Decisions in these areas affect the identity of the discipline: what is not permitted to be communicated officially is defined as not part of the subject matter of the disciplines. Thus, the leaders of sociological societies define some problems as psychology, social work, or something else "not sociological"; the leaders of mathematical societies define some types of work that appear to be mathematics as engineering or physical science instead, and so forth. Related to this control over communication is control over symbolic rewards. Election to high office and the award of a prize for excellent research not only signified appreciation for the work of an individual but confers importance on the kind of work he does.

In most of the basic sciences, university scientists influence decisions about the funding of research at many levels of government and the foundations. The invention of advisory panels and the project system in the United States following 1940 seems to have been very successful; the government has been able to support research without taking away the freedom of scientists or scientific organizations, and it seems as if decisions at lower levels usually allocate research funds efficiently (13) (I have elsewhere criticized the rationality of

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* But see Joseph Ben-David and Randall Collins, "Social Factors in the Origin of a New Science: the Case of Psychology, "American Sociological Review vol. 31 (1966) pp. 451-65. They explain the growth of experimental psychology in Germany in the late 19th century partly as a result of the lack of opportunity for scientists in some of the related biological fields; professorships were not available in such fields as physiology, and some men looked elsewhere. Philosophy, because of its low repute, was not acceptable to them.

decisions at higher levels.*) Ordinarily, the scientific society plays little formal role in making decisions or selecting the personnel for advisory panels, but informally the society can provide a locus for discussions about what research need to be supported and about the right men to be selected for advisory panels. In the area of educational research, scientific societies seem to have had much less influence on governmental and foundation policies than in the established sciences or medicine. This may be due partly to the rather recent growth of government efforts in educational research. It is also possible that this lack of influence stems from a lack of legitimacy of educational researchers in the eyes of relevant publics--the academic disciplines and the intellectual community more generally--and from the great power of educational administrators at local and state levels.

The influence of professions and societies on the conduct of organizations where research is performed is more indirect. If the societies can influence the allocation of prestige among scientific workers, and if they can indirectly influence the decisions of funding agencies, they may indirectly affect the flow of personnel and funds among research organizations. Again, associations in education seem to have been less important in this area than societies in the established sciences, perhaps for the same reasons as noted above.

Despite the relative weakness of professional organizations in educational research, they do have resources to affect the conduct of educational research. In deciding how these resources can be used, choices must be made and dilemmas must be confronted. Six dilemmas for the leadership of the educational research profession can be stated. (14)

1. Boundary Maintenance: Inclusive or Exclusive?

Scientific societies are seldom consciously exclusive, especially in rapidly growing fields which need support and rarely find it desirable to exclude those who want to join. Yet opening a society to all makes it difficult to develop a distinctive identity and fosters dissensus. If the American Educational Research Association were to have significant numbers of members whose primary disciplinary allegiance was to a different field or whose primary occupational affiliation was not to research, its identity would remain blurred. The history of scientific societies suggests that an initial phase of heterogeneity is followed by a phase of purification in which some deviant types are excluded. (15)

2. Internal Differentiation: Disciplines or Problems?

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*See my essay review in Dissent (1968) of Daniel S. Greenberg, The Politics of Pure Science (New York: New American Library, 1968).

A scientific society will at least have competing sessions at its meetings and will probably have separate sections and journals for its members. In the basic sciences this differentiation follows the differentiation of fields of knowledge; in the applied sciences it may follow types of practical problems. The former choice strengthens the divisive tendencies that are already fostered by divergent disciplinary allegiances; the latter choice may make the society less attractive to those with strong basic research orientations.

3. Attracting Social Scientists to Educational Research: Immigrants or Home Grown Products?

The quickest way to involve social scientists in educational research would be to induce already established men to begin research in this area. Such inducement is difficult, and, to be successful, usually requires extensive resocialization and is preceded by painful interdisciplinary projects. Training social scientists in graduate schools of education may reduce such problems, but the products of such schools may be out of the main stream of research in the basic disciplines.

4. Images of the Practitioner: Product Retailer or Product Innovator?

Educational researchers who view themselves as applied scientists may view schools administrators as "retailers" who merely distribute the products developed by the researchers. If this is so, there is little place for the practitioner in the research societies. But if the goals of schools are viewed as problematic, changing, and developing, school administrators may have a more important role to play in associations of educational researchers.

5. Making Decisions About the Allocation of Research Funds: Disciplinary Panels or Practitioner Power?

If the agencies funding educational research move in the direction of those funding physical or biological research, representatives of basic disciplines will come to have considerable influence on the allocation of research funds. These disciplines have conceptions of the proper goals of research, the advancement of knowledge about <u>some</u> aspects of human behavior, which may often be irrelevant for education. On the other hand, if educational administrators influence the allocation of funds for research, the result might be an emphasis upon the evaluation of programs and innovations without considering why they are effective or ineffective.

6. Social Criticism or Institutional Defense?

Educational researchers will be justified by the improvements they help bring about in education, and this accomplishment will require cooperation with practitioners. It is thus natural for researchers to identify with practitioners and come to their defense. Opening the channels of communication in the educational research community to social criticism may divide researchers from practitioners and researchers among themselves. But it may be that some social scientists will become interested in educational research only if such criticism in permitted; only in this way will they see the relevance of their general interests for educational concerns.

The questions stated above are not "dilemmas" in the proper sense, since it is possible to choose both alternatives--mixed solutions can be worked about. Yet the questions represent real alternatives in situations where different values cannot be achieved simultaneously. The alternatives may be consciously chosen or they may be imposed by default. If the latter, the educational research professions may never develop a coherent identity of their own; instead, their values, composition, and goals may be determined by communities of basic social science researchers and of educational administrators.

FOOTNOTES

1. Ralph E. Dunham, P.S. Wright, and M.O. Chandler, Teaching Faculty in Universities and Four-Year Colleges: Spring 1963 (Washington: U.S. Office of Education, OE-53022-63, 1966). The following statistics are taken from this source unless otherwise noted. Essentially the same differences in origins are shown in Lindsey R. Harmon, Profiles of Ph.D.'s in the Sciences: Summary Report on Follow-up of Doctorate Cohorts, 1935-1960. (Washington: National Academy of Sciences-National Research Council, Publication 1293, 1965).

2. Cf. Max Weber, The Methodology of the Social Sciences, trans. and ed. Edward A. Shils and Henry A. Finch (Glencoe, Illinois: Free Press, 1949).

3. August B. Hollingshead, Elmtown's Youth, (New York: Wiley, 1949).

4. Patricia Cayo Sexton, Education and Income: Inequalities in Our Public Schools, (New York: Viking, 1961).

5. Edgar Z. Friedenberg, The Vanishing Adolescent (Boston: Beacon Press, 1959).

6. Burton R. Clark, "The 'Cooling Out' Function in Higher Education," American Journal of Sociology, vol. 65, (May 1960), pp.569-76.

7. Gresheam M. Sykes, "The PTA and Parent-Teacher Conflict," Harvard Educational Review, vol. 23, (Spring 1953), pp.90-91.

8. Neal Gross, Ward S. Mason, and Alexander W. McEachern, Explorations in Role Analysis (New York: Wiley, 1958).

9. Arthur S. Stinchcombe, Rebellion in High School (Chicago: Quadrangle Books, 1964).

10. Cf. Max Weber, "Science as a Vocation," From Max Weber: Essays in Sociology, trans. and ed. H.H. Gerth and C.W. Mills (New York: Oxford University Press, 1946).

11. E.g., James S. Coleman, The Adolescent Society (New York: Free Press, 1961) and other works.

12. Harmon, Profiles: Summary Report on Follow-ups, p.51.

13. See e.g., Charles V. Kidd, American Universities and Federal Research (Cambridge: Harvard University Press, 1959); and Don K. Price, The Scientific Estate (Cambridge: Harvard University Press, 1965).

14. Cf. Herbert A. Shepard, "Nine Dilemmas in Industrial Research," Administrative Science Quarterly, vol. 1, (1959) pp.295-309. There is only a little overlap between Shepard's list of dilemmas and the one that follows.

15. Warren O. Hagstrom, The Scientific Community (New York: Basic Books, 1965). ch. IV.

7

THE NEED FOR FIELD BASED PLANNING: A SUMMARY OF THE COLLOQUIUM

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<u>Of what value is this colloquium? What does it tell us about the community of scholars and developers devoted to expanding knowledge about and improving educational methods?</u>

Its real value hinges on the extent to which two basic propositions have been affirmed.

1. Improvements in substance and methodology in the field of educational research and development are directly related to the ability of communities of researchers and engineers to influence the work of individual members.

2. The controlling mechanisms of the several communities of researchers and developers can be altered.

The colloquium both implicitly and explicitly affirms these two statements. It reveals the interaction between reward systems-money, prestige, advancement-and communication networks, and describes how both of these are imbedded in the cultures within which researchers operate.

A shortcoming of the colloquium was the lack of full consideration of two additional questions: Why be concerned with finding better ways to alter the controlling social and communication mechanisms of a scientific community? What are some new and more effective ways for the controlling mechanisms to operate? There are partial answers in the discussions involved with improving the quality of publications, providing more supportive institutions, and improving annual meetings. But the recommendations are sketchy and unimaginative, making it imperative that more attention be paid to both questions.

<u>Why are new controlling mechanisms needed for the educational R & D communities?</u>

Since educational research and development is rooted both in the disciplines and in an applied field, it follows that the community is highly diverse. A group this complex has tenuous ties. Keeping well monitored information flowing across several disciplines is difficult enough; diffusing it further to developers and engineers compounds the problem. So one answer to the question "Why are more effective mechanisms needed?" is to make this multidimensional community work as effectively as possible. This is a job that demands constant attention.

But there are other reasons. Society is now making greater demands on virtually all segments of the behavioral and social sciences, not just the applied engineers. The disciplines are being asked to devote greater attention to building the kind of knowledge that will help solve environmental problems, alleviate crime and poverty, improve the way we are governed and the way we educate ourselves at all ages. Philip H. Abelson states:

> The goal of opinion-making should be constructive action. A prerequisite for this is thorough planning based on an adequate fund of knowledge. Scientists can make imaginative contributions to planning, and they can help ensure that the factual bases for decisions are as sound as possible. (1)

The demands are overwhelming and the resources so meagre, especially in educational research.

In 1964, according to Clark and Hopkins, (2) there were approximately 4,125 persons available to do the research, develop new products and diffuse the results throughout the educational enterprise. Most of the research producers were concentrated in a handful of universities. The typical researcher was an isolated individual with little or no released time for his studies.

In the past few years, this relatively small group has been called upon to staff new institutions like the regional educational laboratories, research and development centers, national laboratories, and a continuously increasing number of institutes and centers at individual universities. They also have been called upon to help hundreds of school districts and newly established service centers evaluate their dispensation of federal funds. While there have been additional funds provided to train educational researchers, the supply has not come close to meeting the demand. (3) This has resulted in stretching the talent in the educational R & D field far beyond desirable limits.

There appears to be no letup in sight. Therefore, the second explanation for asking the community to function more effectively is to help increase the productivity of the limited, available talent.

A third reason is to accommodate what, at the colloquium, William Paisley called "the rising expectations" for societies. Young scholars, many of them members of minorities, are demanding that their respective communities of scholars or scientists, through their societies, take a more active role in shaping public policy. They want societies to use their mechanisms to influence the direction and priorities of fields of study. Voluntary membership groups are re-examining their functions and are taking on new ways of operating.

There is still a fourth reason. The remarkable accommodation envisioned by Vanevar Bush (4) and used for the past two decades by the

Federal Government has become seriously endangered within the past two years. The price of big science has placed it in competition with action programs and Congressmen get more votes for helping their constituents. Our country does not have endless resources, and scientific research by itself lacks the "pizzaz" to keep it competitive with other governmental programs.

The result has been the demand that science become more relevant, that it turn its attention primarily to solving the nations' ills. Those who want to continue to advance knowledge are advised to "bootleg" very limited funds in with some applied program. Or they are admonished to learn the political processes, to get their hands dirty and lobby for their funds.

Neither answer is satisfactory. Certainly basic researchers should not expect to utilize public money to play games or simply to enhance their bargaining power in the academic marketplace. But they must be permitted to pursue seriously questions that, on the surface, may not appear to improve directly the health or well-being of man.

In previous years, the science program managers within the bureaucracy adequately represented the needs of basic research. But scientists today are discovering that, in times when money is scarce, the research managers cannot carry the arguments without strong, effective support from the outside. Researchers in the field must find new ways to determine how much financial support is needed and what form of subsidization will best continue to advance basic knowledge. Some of the shorthand methods used in the past (percentages of the total appropriation in a field or a ration of funds for development to that of research) will no longer win votes on Congressional appropriations committees.

Yet it is not just basic research that must be documented more precisely. Assigning priorities among the various research programs funded by the Federal Government has become a very complex process. Why should more funds be given to the study of education rather than to the study of the environment or of crime? Within any field as broad as education, how are priorities assigned? What determines that the study of early childhood education should take precedence over the study of early adolescent education or young adult education? Or the study of reading over the study of mathematics?

In response to these and similar questions, the Federal Government is developing new ways to evaluate existing programs and to maximize the productivity of given efforts. Some of the better known approaches are the Program Planning and Budget System (PPBS) and Planning, Evaluation and Reporting Technique (PERT). But there is an underlying emphasis upon the more systematic collection and use of data in planning, monitoring and evaluating federal programs. Anyone who has read through some of the highly detailed documents that are prepared by governmental staffs can understand how important data has become.

So the government planner and policy maker increasingly is using data to determine answers to policy questions. (5) He expects outside advisors to do the same. He will ask why the researcher wants more money for basic knowledge. How will it be used? What is it likely to produce? The days are fast receding when a scholar or researcher can expect his personal opinion, formed from his individual experience, to answer questions like these.

It is my contention that men in the field - that is, the researchers and scholars who are primarily responsible for developing new knowledge and helping the practitioner to better understand how to use this knowledge - need more sophisticated ways of discerning how to use given resources to accomplish the most desirable ends. These procedures should be different in kind but just as sophisticated as those used by the bureaucrats. I shall call them field based planning.

Field-based planning - what is it?

What is planning? Kahn (6) describes it as an intellectual endeavor that seeks to analyze problems or needs as closely as possible, set objectives, and determine the resources available to achieve those objectives. In other words, it is a rational approach to gaining maximum contribution in a complex field of study. It is needed, he says, to seek as much understanding as possible of the organizational activities which will result if there is no intervention.

This kind of planning can take many forms. In educational research it must essentially be concerned both with the planning for knowledge and for the maximum utilization of that knowledge. The two functions call for different tasks.

There is considerably more assessment of the state of knowledge in most fields of research than generally is recognized. Take educational research where there is probably less review of the growth of knowledge than in the more established disciplines. The Encyclopedia of Educational Research, occasional books like the Handbook of Research on Teaching and the Handbook of Research on Child Development, the periodical Review of Educational Research, and the Annual Review of Psychology are only five of the better known means through which the research community takes stock of current knowledge. The annual meeting of the American Educational Research Association has shifted its emphasis over the past four years towards reporting on the state of the art in those specialties where most work has been done.

As valuable as these reports are, they don't meet some of the most essential requirements for field based planning. Reviews devote most of their attention to analyzing work that has been completed. Some attention is given to analysis of problems that remain. But seldom do the authors talk in terms of goals that must be reached if the state of knowledge is to be advanced significantly. Let me take only one example.

In their postscript to the chapter "Developmental Psychology", Campbell and Thompson make the following statement:

> Two parts of Piaget's scheme remain relatively unexamined: the period of formal operations (ages eleven to fifteen) and his notions about cognitive motivation based upon the need to obtain an equilibrium between mental structures and the environment. (7)

The reader is left with an unanswered question: Are these work essential to the development of theory or working experimental models in the eyes of the authors? If so, why don't they say so? Contrast their comment with one by Hunt that appears in the same volume in the chapter on computer simulation:

> If computer programs are going to be proposed as models of behavior, more attention should be paid to showing that the programs really do simulate.
> In fact, I do not believe that this is going to happen. While there will undoubtedly be studies of verifiable computer models, especially for experimentally manageable tasks, the stress appears to be more and more on "process research", studying the behavior of problem solving algorithms for their own sake. This is perfectly legitimate, but is ancillary to psychology. Computer programming seems to be a more appropriate tool for studying the broad implications of a proposal for how one should think than for realizing a testable model of how one does think. We cannot expect to find realistic models by this use of computers, but we can expect to uncover important facts about problem solving per se which will, and which in some cases have, influenced our thinking about thinking. (8)

Here the author has made a clear statement of what, in his opinion, are necessary goals. Such statements are essential for setting and realizing objectives.

But so far we have limited the planning function to written comments about substantive and methodological developments and needs. Planning also must consider how the changes will be engineered. This may take the form of the "set of strategies for integrating the psychology of learning with the technology of instruction" (9) proposed by Hilgard or the logistics of integrating research, development and practice proposed by Glaser in the same volume. (10)

Writing reports of this kind would be extremely difficult. While the task must be assigned to specific individuals, the author will need a group of his peers to help him critically assess the present state of knowledge, to make prognoses about trends and to take stands on what else is needed. Where agreements are not possible, points of view attributed to specific individuals can be noted. The final product should remain attributable to the work and the talents of an individual but the points of view could reflect the reactions of several knowledgeable specialists.

What might these reports look like? Nothing that exists, although some parts of the reports from the Behavioral and Social Sciences Survey (11) are applicable. The first section would report on significant trends and developments that become apparent in the preceeding 12 to 18 months in educational research and related fields. This, in some ways, would be a condensation of what now appears in the five issues of the Review of Educational Research and of relevant chapters from the Annual Review of Psychology. (12) The next section would discuss the implications of these trends and developments. Should research now be more sharply focused on the importance of teachers' expectations of student performance as a result of Robert Rosenthal's work (13)? Or should that study be replicated as rapidly as possible using different statistical treatments? When the first results of the commission on Assessing Progress in Education are published, what effects will these have on the methodologies used in pupil assessment? On the concept of assessment? Should researchers begin duplicating these studies quickly? Or should other ways for determining national norms of educational progress be tried? These are only illustrative questions that could be used as guides.

There is still another section needed, however. The commission should be asked to comment on the implications of their prognoses in terms of the available manpower, its competencies, the availability of funds and the institutional support for educational research. Suppose a member of the commission believes that the Coleman report on the Equality of Educational Opportunity points out the need for more studies to better determine the relative influence on children of the school, family, peer society and other factors. One of his recommendations might be that the Office of Education, perhaps through the Committee on Basic Research of the National Research Council, become more active in encouraging sociologists to study schools. More fellowships for sociologists interested in education also could be advocated. And AERA could be asked to organize training sessions and special meetings to more rapidly exchange information and methodologies among sociologists, anthropologists, psychologists and educationalists.

The commission's report should be carefully packaged and given wide distribution throughout the field and the Federal Government. It should be read by the governing councils of societies, by university officials, and federal program policy planners.

Planning for the application of knowledge

But what about field based planning at the other end of the continuum, for the application of knowledge? The approach here must be different. The status of the diffusion and application of knowledge will be much more difficult to ascertain, for it involves both the development of new products, systems and techniques, and the shaping of opinions and beliefs of practitioners. Let's examine each separately.

A survey should be conducted bi-annually to catalogue the newest products, systems, curricula or practices that have been designed from a sound conceptual base or that are being subjected to systematic testing and evaluation. For example, the IPI program, Individually Prescribed Instruction, contributes to and furthers knowledge about the individualization of teaching. Some of the curricula designed to teach English as a foreign language to Spanish speaking children applies and tests psycholinguistic principles. Yet unfortunately most new curricula revisions are not rooted in sound educational theory nor are they systematically tested.

The survey should be conducted, and the data analyzed, by a small staff of persons who also would be responsible for publishing annual, or semi-annual, reports. They will be advised and assisted by a commission of knowledgeable researchers and eminent educational statesman. The report would resemble the document prepared in basic research, but with some important differences.

One section of the report would focus much more on the impact these new products are having on procedures and practices in the schools, with an analysis of the impediments to change and the factors that encouraged change in specific settings. Another could discuss how these products could be diffused more extensively.

But products are not the only results of research. All too frequently the impact that research and scholarship have on the generally held beliefs of practitioners is overlooked. The theory of evolution has had momentous consequences far beyond the use for which Darwin's writings originally were intended. So has Werner's conception of socio-economic stratification of societies. The theories of learning and teaching held by teachers shape their behavior in the classroom. If they believe, for example, that students who have high intelligence test scores but who perform poorly in the classroom are just "lazy", they hold a fundamentally different belief system from teachers who see the same students as,say, poorly motivated.

An assessment of the theoretical beliefs held by educational practitioners should be conducted on a scale and should use techniques similar to those recently employed by the Commission to Assess Progress in Education. This belief system inventory should be conducted bi-annually so that shifts and trends can be traced. (14) The educational research community also can derive clues as to the relationship, if any, between research findings and changes in these theories and belief systems.

Who should be responsible for the planning?

Scientific societies are well equipped to take on field based planning. They have many of the mechanisms needed to intervene- publications, meetings, training sessions - plus accessibility to the leaders in their respective fields. As a matter of fact, the elected officers of many societies could devote more time to planning. Most organizations spend far too much time haggling over insignificant details of management - how to get more members, how to improve the promotion of publications - and far too little on matters of vital concern to the field being represented.

Since scientific societies typically keep the bureaucracy small and concerned with the logistics of their activities, a shift to the planning function would necessitate changes not only in the behavior of the governing boards but also in the characteristics and size of the central staffs. What are the alternatives to a complete structural overhaul?

Associations might create separate organizations patterned after the National Research Council - National Academy of Sciences relationship. This would enable them to operate traditional functions like annual meetings, publishing technical, small circulation journals, and bestowing honors on their members. The planning staff could be housed in the NRC organization.

Smaller organizations might band together to support the separate staffs. In educational research, such a group might be financed by the National Academy of Education, AERA, the National Council for Measurements in Education and the National Council for Research in Science Teaching.

Financing is a serious stumbling block to the evolution of field based planning. Dues to organizations now are high and going higher just to support what can be termed traditional functions. Considering that the typical researcher belongs to several organizations, it is not uncommon to find him now spending from $75 to $150 for dues; it will be difficult to get more money out of him for tasks that, at first glance, do not seem to be related to his primary job. Societies must find other sources of income.

Immediately one thinks of the federal government and private foundations for grants. These sources should not be overlooked, especially the government. Too frequently federal agencies try to forge new channels of communication for such tasks as diffusing information to researchers and engineers when they could get better results, possibly for less money, by using the existing networks of voluntary organizations. Associations should stress the potential of such joint action. But associations also must look to their own resources.

Most societies and associations operate under a debilitating and outmoded financial posture, one that was appropriate only in the days when they did little but hold annual meetings and publish technical journals. Societies today should abandon the concept of "not for profit" and instead attempt to make as much money as they can from every activity they sponsor. Already they are being called upon to do more and more in the next several years. Such increased activity can be realized only when a surplus of funds is available. The cash "profit" would not cause any internal revenue problems because the funds immediately would be committed to additional activities. In some ways we might think of this practice as a form of capital investment.

How might additional income be raised?

Presently annual meetings are subsidized through dues. They could, however, become profit-producing. Registration might be thought of as tuition; special sessions featuring famous men should require additional fees; printed materials, like books, should be sold at convention "bookstores".

The same principle could be followed for smaller meetings or training sessions. In fact, certain meetings should be held for the purpose of raising money. Associations should not only consider how best to market their publications but also how they might package certain materials to produce income. Two principles must not be violated, however: keep the fund-raising activities in perspective so the primary mission of the association is not diluted; and be sure that whatever is produced serves the target audience as well as possible. High quality must be maintained at all times. Then, and only then, are these kinds of fund raising activities justified.

What is advocated here is a major shift in the function of societies. It is a proposal that they move off the sidelines and take a central role in the development of their field. Only in this way can researchers become more effective both in shaping the substance of their fields and in influencing relevant aspects of public policy.

FOOTNOTES

1. Philip H. Abelson, "Social Responsibility of Scientists", Science, Vol. 167, (Washington: American Association for the Advancement of Science, January 17, 1970), p. 241.

2. David L. Clark and John E. Hopkins, A Report on Educational Research, Development, and Diffusion Manpower, 1964-1970, mimeographed, (Bloomington, Indiana: Indiana University Research Foundation, 1969), pp. 105-6.

3. Ibid., pp. 297-373.

4. Vannevar Bush, Science the Endless Frontier: A Report to the President, (Washington: U.S. Government Printing Office, July 1945).

5. See, for example, "Nixon's White House Staff: Heyday of the Planners?", Science, Vol. 167, (Washington: American Association for the Advancement of Science, January 17, 1970), pp. 232-4.

6. Alfred J. Kahn, Theory and Practice of Social Planning, (New York: Russell Sage Foundation, 1969), p. 13.

7. Dugal Campbell and W. R. Thompson, "Developmental Psychology," Annual Review of Psychology, Vol. 19, (Palo Alto, California: Annual Reviews, Inc., 1968), p. 282.

8. Earl Hunt, "Computer Simulation: Artificial Intelligence Studies and Their Relevance to Psychology", Annual Review of Psychology, Vol. 19, (Palo Alto, California: Annual Reviews, Inc., 1968), pp. 160-1.

9. Ernest R. Hilgard, "A Perspective on the Relationship Between Learning Theory and Educational Practices", Theories of Learning and Instruction, (Chicago: The National Society for the Study of Education, 1964), pp. 411-15.

10. Robert Glaser, "Implications of Training Research for Education", Theories of Learning and Instruction, (Chicago: The National Society for the Study of Education, 1964), pp. 176-81.

11. Behavioral and Social Sciences Survey, (Washington: National Academy of Sciences, 1969).

12. see Annual Review of Psychology, (Palo Alto, California: Annual Reviews, Inc.).

13. Robert Rosenthal and Lenore Jackson, Pygmalion in the Classroom, (New York: Holt, Rinehart and Winston, 1968).

14. Work of those, such as Eleanor Sheldon and Wilbert Moore, who are developing social indicators could be used as guidelines.

The Educational Research Community: Its Communication and Social Structure

SUMMARY

The AERA colloquium was held to better enable the educational research community to assess and deploy its resources, coordinate research efforts, obtain more effective communication within the field and beyond it. To do this researchers used their professional association to reach out to another speciality, the sociology of science, to find models in other fields of scholarship.

Several outstanding scholars were invited to present papers on their studies of the social systems and communication networks of scientific communities and to help elucidate the nature and characteristics of the professional behavior of educational researchers. William Garvey and his associates described the more typical communication channels in educational research. William Paisley and David Lingwood examined the concept of invisible colleges as a way of examining the community of educational researchers. Ronald Corwin and Maynard Seider reported on their interviews of scholars who have examined the institutionalization of science. Warren Hagstrom and Norman Storer each examined the educational research community using conceptual frameworks that have evolved from their studies of the social structure of the hard sciences.

These authors were joined by selected leaders within the educational research community who discussed the papers and made recommendations to the research community in general and to AERA specifically.